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PART I.

ORIGINAL COMMUNICATIONS.

ART. XI.—*On Amputation through the Knee-Joint.* By WILLIAM MAC CORMAC, M.A., M.R.I.A. ; Fellow of the Royal College of Surgeons of Ireland; Member of the Senate of the Queen's University; and Surgeon to the General Hospital, Belfast.

AMPUTATION through the knee-joint or, more correctly speaking, disarticulation is an operation of very ancient date. The very oldest authors allude to it. Indeed, it would naturally suggest itself as a ready mode whereby to remove a diseased or injured limb. That remarkable surgeon, Fabricius Hildanus, to whom are attributed many great surgical achievements, is alleged to have performed this amputation in 1632, and he speaks of it as an acknowledged procedure amongst contemporary surgeons. Hoin of Dijon, about 1764, performed the operation, by means of a long posterior flap, similar to that more recently recommended by Mr. Syme. J. L. Petit, amputated after a similar fashion on two occasions, once, it is said, disarticulating because there was no saw at hand wherewith to divide the bone.

Blandin, who followed Hoin's plan, recommended a counter opening at the base of the long posterior muscular flap for the escape of pus. He mentions as a serious objection that the portion of the soleus muscle, which helps to form the flap, always mortifies.

Brasdor especially extols the operation in an elaborate memoir, published in the Transactions of the French Academy of Surgery, where, amongst other matters, he points out how the circular, or transverse operation as Brasdor named it, may be modified by taking more skin from the front of the limb, according to the seat and extent of the injury. He thus indicated, in short, the modern form of operation.

To Velpeau, however, is conceded the claim of having introduced amputation through the joint into modern surgery. In 1829, he published a memoir enthusiastically commending it, citing fourteen cases, out of which there were thirteen cures. His subsequent experience led him to modify the sanguine expectations he had at first framed respecting disarticulation at the knee, but he maintained to the last the excellence of the operation. Malgaigne expresses the opinion, in his *Manual of Operative Surgery*, "that the operation has been too lightly abandoned, that it possesses every advantage over amputation in the continuity of the shaft of the femur, especially that of leaving the patient freedom in, and command over, the motions of the hip-joint." More recently still American surgeons have devoted much attention to this subject, notably Drs. Smith, Markoe, and Brinton. In our own country, Mr. T. Holmes and Mr. Pollock of St. George's Hospital, and several others, have recognized the merits of this form of surgical procedure. Mr. Samuel Lane was the first, I believe, to perform the operation in England, which he accomplished most successfully in 1857. Before this it had been done in the Glasgow Infirmary in 1847. I do not know to whom in Ireland is to be attributed the first disarticulation of the knee-joint. Amputation close to the joint is not infrequent.

Amongst the advantages which Dr. Markoe, in one of his valuable contributions, claims for disarticulation at the knee are:—

1. The useful character of the resulting stump.
2. The small shock attending the operation.
3. The lessened section of the tissues.
4. The non-exposure of the muscular interspaces and diminished risk of suppuration in the thigh.
5. The comparatively few vessels requiring ligature.
6. The attachments of the thigh muscles being mainly preserved.
7. The absence of muscular retraction.
8. The diminished risk of osteomyelitis and exfoliation, from the femur not being sawn across.

And here it may be mentioned that under the term "amputation

at the knee," two different operations have often been confounded, operations different in character, and likely to differ, as will afterwards be shown, in their results. These are, true disarticulation of the joint, without interference with the articular surfaces of the femur or patella, and amputation through the condyles of the femur, removing more or less of that bone, and, generally speaking, removing the patella also.

Of the latter form are Syme's amputation at the knee, and Mr. Carden's modification of it. Deploring the great fatality of thigh amputations, estimated by him at from 50 to 70 per cent., Mr. Syme re-introduced Hoin's operation, recommending the femur to be sawn through the condyles in order to avoid the many risks which he attributed to dividing the medullary canal. The long posterior flap made of the whole of the calf was nevertheless attended by so many and so serious inconveniences that Mr. Syme felt constrained to abandon this mode of operating. Mr. Carden, of Worcester, some time after, advocated a similar operation so far as the bone is concerned, but he made a long anterior integumental flap, sufficient to cover with ease the entire of the cut condyloid surface. By this plan, which Mr. Carden, at the time of publication, had followed for many years, he achieved marked success, and Mr. Syme acknowledged the method to be a great improvement upon his own. In both these operations the patella is removed.

This form of operation, then, is very different from disarticulation at the knee where, when sound, the cartilages of incrustation on the end of the femur, and on the patella are not interfered with.

The method of amputation through the joint employed at different periods, has, as already indicated, varied much. The long posterior flap of Hoin and Blandin would never now be accepted through choice. Its weight and liability to gangrene, added to the difficulty of keeping it in situ, are sufficiently strong objections, and there are others as weighty. Velpeau adopted a circular cut, four fingers breadth below the joint. This is, however, inconvenient, and leaves a cicatrix on the face of the stump. This latter, and strong objection, subsists against the otherwise excellent operation by two lateral flaps. The operation practised by M. Baudens approaches very nearly the plan now most generally accepted. This eminent French surgeon recommends an oval sweep to be made round the limb extending in front five fingers' breadth beneath the inferior border of the patella, and posteriorly three fingers' breadth below this level. Twice successfully did M. Baudens operate in this

way. In one instance the cartilages were not interfered with, and the stump healed over without any exfoliation taking place; in the other the condyles had been injured by a musket ball, and required removal. Dr. Nathan Smith successfully performed this amputation for the first time in the United States in 1824, and therefore before Velpeau. He removed the limb by forming two flaps anterior and posterior, exactly equal in size. The base of the flaps was half an inch below the line of the articulation, and their extremity corresponded to the insertion of the ligamentum patellæ. Dr. Stephen Smith in the last January number of the *American Journal of Medical Science* recommends a very ingenious form of incision, which for this and other operations is well worthy of consideration.

Such, then, are some of the ways in which disarticulation at the knee has usually been practised heretofore. In quite recent times, in the opinion both of American and English surgeons, one plan of operation is preferable, when at all practicable, to all the rest, and that is a long anterior tegumentary flap, and a shorter posterior flap, while, when healthy, the cartilages of incrustation on the femur, and the patella should remain absolutely untouched. This was the method I myself adopted when I had occasion to perform this operation.

The points of importance in connexion with it are that the anterior flap should, at its base, be nearly or quite two-thirds of the circumference of the knee, just below the patella, and that its length should correspond to at least half its breadth. Further, that the incision to form this flap should commence and terminate on the inner and outer sides of the limb, one inch below the line of the articulation in the adult. To express this in another form, the anterior flap should commence opposite the posterior margin of one condyle of the femur, and extend across the limb about four or five inches beneath the patella to a similar position as regards the other condyle. The superior extremities of this incision should be an inch below the semilunar cartilages. This flap having been dissected up so as to include all the soft tissues in front of the knee, the flexed joint should now be opened as near the femur as possible. By this means all superfluous synovial membrane is cut off, and the semilunar cartilages are not separated from the tibia, as they might otherwise readily be, thus necessitating a separate dissection for their removal. The crucial and lateral ligaments are now with facility divided, the knee being still maintained flexed. As soon as these are freely cut, the knife should be laid flat behind the tibia,

the limb extended, and a thin posterior flap, about half the length of the anterior, rapidly formed by transfixion. Care must be taken in so doing to avoid locking the edge of the knife against the projecting head of the fibula. The posterior flap may be made integumental merely by dissecting it up from without, as in making anterior flap, and then dividing the rest of the soft parts by a circular sweep of the knife. Very few arteries require ligature. I have, however, always found it expedient to tie the popliteal vein. It is of the utmost importance to avoid heavy or tight dressings. These should be always very light, and any tension promptly relieved, should it take place.

Having said so much by way of introduction, which I trust may not prove too tedious, I now purpose briefly relating the particulars of four cases, in which I performed disarticulation of the knee, and of one case in which I performed Carden's amputation.

Case I.—The first case in which I amputated through the knee was that of a man aged thirty-one, admitted under my care to the General Hospital, April 8th, 1865. For three years previously he had been suffering from pain in the left knee, more severe at night, and on putting the foot to the ground. No other symptom was complained of for two years, when some swelling appeared and the pain grew worse. Only three weeks before admission was he compelled to give up work. When I first saw him there were considerable swelling in the knee and severe pain. Appropriate treatment was ordered, under which he improved very much for a time. On May 31st very serious symptoms suddenly supervened. The patient had a rigor, was very feverish, the leg became œdematous, and the joint swelled. Acute suppuration in the joint, it was soon seen, had taken place. On June the 5th a free opening was made, and a large quantity of matter flowed from the joint. On introducing a probe it passed into softened cancellated tissue of the head of the tibia, to which bone the symptoms were all along chiefly referable. Although a very hopeless operation in similar cases, the patient's general condition was such as led me to propose immediate amputation. Unfortunately he would not hear of it until June 10th, when his state was very bad, but he then urged me to relieve him were his chances never so bad, and after a consultation I did so, although some symptoms, not unlike those ushering in pyemia, had been manifested. The patient being chloroformed I made an oval anterior flap six inches long, opened the joint and made a short

posterior flap by transfixion. There was pus in the joint. On examining the end of the femur it was found that the cartilage had been removed and replaced by granulations. No diseased or softened bone was felt, and it was decided to be better not to inflict further injury on the femur by slicing its end off. The patella was similarly affected, and it, too, was left untouched. The disease had commenced in the head of the tibia, to which it was mainly confined, the femur being only secondarily affected. An abscess had formed in the head of the bone which afterwards found its way into the joint, and the action in the femur was an effort to protect itself from injury. The popliteal artery and one other vessel were secured by acupressure needles (inserted after Simpson's third method). The wound was then drawn together by sutures, and the patient removed to bed, water dressing having been applied. As might be expected there was considerable tendency to general oozing, very difficult to check, and returning two or three times. The application of ice was had recourse to. An examination of the part removed justified the diagnosis. A pyogenic cavity was found in the cancellated structure of the head of the tibia. Around this the bone was softened and inflamed, and a round opening communicated through the centre of the external articulating facet of the tibia with the joint. It was, no doubt, in consequence of this perforation suddenly taking place that the violent inflammation all at once occurred in the joint. The day after the operation the symptoms of pyemia in its most acute form set in. Violent rigors, hot fits, and sweating of the most profuse kind, induced me to anticipate a speedily fatal result. No treatment is of much avail in acute pyemia. I simply administered large doses of opium. I need not weary the reader by a detailed history of the case, further than to add that on dressing the stump on the third day, the anterior flap was observed to be fringed by a gangrenous border. No healthy action ever took place in the wound. No clot formed in the popliteal artery, which pulsated violently in the open sloughing wound. Fortunately the needle kept its place even under these unfavourable circumstances, showing that it does not cut the arterial coats so speedily as some has alleged. Harbinson died on June 16th, exhausted by the disease. The operation, perhaps, hastened the fatal issue, but that even is doubtful, as for three or four days he asserted he had been greatly relieved by it. That his life would have been saved had he assented to an operation when it was first proposed to him is perhaps unlikely, but, beyond question, a better

prospect of recovery would have been afforded. No general *post mortem* examination could be obtained, but I seized an opportunity of examining the stump. Unluckily the dressings had been displaced during the removal of the body to the dead-house, and the needle on the popliteal artery torn off with them. The muscles were dark in colour, and the popliteal vein was filled with thin greyish pus. There were two small secondary abscesses in the thigh.

Case II.—On the 24th May, 1866, the anniversary of the Queen's birth-day, I was summoned some miles to the country, by Dr. Fulton, of Saintfield, to see a poor man called Quin. He was a commercial traveller of very intemperate habits, and about fifty years of age. Whilst celebrating the anniversary by firing off a small cannon for the amusement of his children its contents accidentally lodged in his left leg. The cannon was eighteen inches long, and of two inches bore. The charge consisted of six or seven ounces of powder, in addition to which he had foolishly introduced a quantity of small stones, picked off the walk in front of his door. It is supposed that the cannon swerved just before being discharged. At all events, he received the entire contents in his leg.

The result was a horrible lacerated wound, six or seven inches long, and the entire breadth of the limb. Above, it reached nearly to the tuberosity of the tibia. Both bones were extensively comminuted, and a piece of the tibia two inches long was lying loose in the centre of the wound. I thought it likely the fracture extended into the joint, but could not make myself certain of its doing so. The soft parts were completely disorganized, and blackened by the proximity of the poor man to the discharge.

The question of saving the limb was not entertained, the only point to determine was where to amputate. Considering the nature of the injury both to the bone and soft parts, I at once decided against amputation in the leg. The bone was probably split up into the joint, and the soft parts were so much bruised and lacerated that I thought they were not safe to furnish sound covering, and this could only be obtained from the calf. I decided on amputation near the joint by Carden's plan. Had the covering to be obtained been sufficient I would have preferred disarticulation. An operating table was extemporized, some candles provided, and the patient being chloroformed, I made an anterior flap as long as possible, down in fact to very near the margin of the wound. This

flap was dissected up, the joint opened, the crucial and lateral ligaments were divided, as also the tissues posteriorly by cutting almost directly outwards, I then sawed a slice off the condyles. The popliteal and the azygos articular arteries alone required ligature, unless we can reckon a small sural branch. I also passed a ligature round the popliteal vein, as the bleeding from it was not arrested by pressure, and I have no reason to believe that the ligature of a vein in a stump is hurtful, however large. Interrupted sutures and water dressing having been applied, the patient was removed to bed. I dressed the stump on the third day. Union had taken place between the flaps on the right side, but elsewhere a very unhealthy discharge was being poured out. The thigh was mottled all over with yellow and purple patches. The conjunctivæ were yellow. The pulse was 120, and although he had slept well during the night, he continued in a somnolent state during the day. It was at this time that the sulphurous acid treatment, recommended by Professor Polli, was being tried in septic diseases. Fearing the advent of pyemia, although the patient was located in a country cottage, and not in a crowded hospital, I ordered the bisulphite of soda in thirty grain doses frequently repeated, as a prophylactic, in which capacity, this, as well as some other remedies, has been alleged to act admirably. I also ordered ample nourishment, and a moderate quantity of wine.

Next day, the 28th, there was to be seen at the edge of the anterior flap a portion, which appeared to be losing, if it had not already lost its vitality. On the 29th the pulse was 120, the tongue was clean and moist, there was a plentiful discharge of thin unhealthy pus. On examining the stump, the anterior flap was found to be fringed with a gangrenous margin half an inch deep and three inches long. This did not extend any further, but the symptoms of pyemia increased, the jaundice persisted, the breath acquired a sickening sweet odour, he had severe sweating, the belly became greatly swollen and tympanitic. The stomach rejected everything, and uncontrollable diarrhea set in. He gradually, in short, became weaker, and died on the 6th of June, a fortnight after his mishap. The double shock of the accident and operation, and above all, the man's antecedents, contributed to cause the unfortunate termination. Probably even a smaller amount of injury, or a less serious operation would have been followed by a like result.

Case III. presents many features of interest, apart from the

method of operation adopted. It is that of the young man whose photograph accompanies this paper.

William J. Allen, a tall lad, seventeen years of age, rather delicate looking, and with a very anxious cast of countenance, was admitted to hospital, under my care, in last October, with a large tumour occupying nearly two-thirds of the right tibia.

He states that some eight years ago, when only nine years old, a small swelling which has uniformly, though slowly, increased ever since, formed over the tibia. Within the last twelve months the rate of growth has been more rapid. There was never much pain, and generally no pain whatever, attending it. The tumor occupies the inner and anterior aspect of the tibia. Above, the tumor terminates four inches below the head of the bone, below it extends within five inches of the ankle. The tibia above the tumor is not quite free from enlargement, below it very much increased in size. The surface of the tumor is smooth, of bony hardness, and channelled by the superficial veins. It rises gradually up from the bone, which seems clearly to be expanded over it. On the lower and front part is an indolent superficial ulcer about three inches square. This began some fifteen months before, and has since been slowly enlarging. It is not painful, and the amount of discharge from it is very trifling. It seems to have been originally produced by the friction of the clothes against the part.

The diagnosis, though not quite simple, indicated a bony or cartilaginous benign tumor growing in the bone, which was expanded over it.

After consultation it was decided to amputate. The boy could, no doubt, walk about pretty well, there was no swelling in the foot, he complained of no pain, nevertheless his general health was beginning to suffer a little, the tumor was increasing more rapidly, and the lad himself being most anxious to have the leg cut off, I could see no advantage in delay.

The operation was performed on November 6, under chloroform. A long anterior flap, nearly down to the tumor, was first made. The incision was commenced on the inner side, three quarters of an inch below the line of the articulation, and well back near the hamstring tendons. The cut terminated at a corresponding point on the outer side of the joint. This flap was rapidly raised up, the joint opened, bisected, and a thin short posterior flap formed, after the limb had been extended, by cutting from within outwards. With

the exception that it is placed just a little too low down, the line traced on the cast in the photograph represents the direction of my incisions. This cast was taken before the limb was removed, and gives a good idea of the form and size of the tumor. I thought it would lend further interest to the picture if it were photographed at the same time as the patient upon whom the operation for its removal had been executed. At first sight it would appear that these flaps are made needlessly long. Such is not the fact, the amount of retraction is considerable, and if they were made shorter great straining must result, as the flaps often retract very much. If you find the covering insufficient you will then have to perform Carden's operation by cutting off the condyloid end of the femur. This is what I felt compelled to do in Quin's case. In the present instance by the time the popliteal and four other vessels had been tied, the patella was drawn up some two inches, and when the flaps were brought together they met at the posterior part of the stump without traction certainly, but also without the smallest redundancy. Neither on the femur nor on the patella was the cartilage interfered with. Carbolized dressings were applied to the stump after the flaps had been washed and sutured. Two days after the operation, the patient having been going on well in the interim, his pulse rose to 120, he became feverish, and complained of great pain in the stump. Fearing there had been some oozing of blood, distending the flaps, I partially slit up the dressings, through a space of some inches, at each angle, found the edges of the wound firmly adhering together, and no appearance of distension. This allayed my fears, and I did not disturb the dressing further. The pain reappeared on the fifth day, and I then removed all the dressings. The central portion corresponding to tip of the long flap had become gangrenous to the depth of three-fourths of an inch, by two inches broad. I came to the conclusion at the time that this was perhaps induced to some extent by the pressure of the bandages. A flap so long and delicate requires the very gentlest treatment. I believed, however, I had taken all needful precautions. No extension of the gangrene occurred. The slough was soon thrown off, and the condyles became exposed.

On December 7, a month after operation, I find the following record, which I transcribe from my note book:—" Since last report this patient has been getting on in all ways well. A superficial healthy granulating surface occupies the site of the former slough. All the rest of the wound healed by primary adhesion, and has

since remained sound. It was interesting to watch the gradual process of removal of the exposed cartilage. It seemed precisely analogous to the process, only somewhat slower, which occurs when some part of the cranium is denuded of periosteum—a layer of the skull perishes, and is disintegrated slowly, or scales of bones are thrown off. Granulations form from the sound bone beneath, press upon and absorb the dead portion, or it may be wholly surrounded by granulations, loosened, and cast off in a piece.

"I carefully watched similar changes going on in the exposed cartilage. It seemed gradually to get thinner, and around the edges, granulations springing from the surrounding parts encroached. After a time granulating points appeared through the cartilage itself, rising from the bone beneath. These points extended, and the granulating surfaces gradually fused together into one tract, and all trace of cartilage disappeared. The process is one of slow absorption and substitution. In other cases the cartilage may be thrown off in large exfoliations." "At the present date," the report continues, "any amount of pressure can be sustained by the condyloid surface. The patella remains quite movable, and is drawn up at least two inches above its normal situation. The lad goes daily about the hospital, and out of doors, and is, in fact, in all respects convalescent." He left the hospital soon after. The photograph was taken in the middle of March, about four months after operation.

With a detailed account of the nature of the tumor I need not trouble my readers. The tibia was expanded over it. While above, and especially below, the bone was much hypertrophied. It was in addition two inches longer than the tibia of the other leg. The fibula, though not involved in the growth, was hypertrophied. On making a section of the bone it was found to contain a number of small and large membrane-lined cavities, varying in size from a pea to a walnut, containing bloody serum, and divided from each other by thin bony walls. In the interstices were masses of yellowish white substance, firm and elastic. In some places these were softer and opalescent. A microscopic examination showed this material to contain multitudes of cells of all sizes, spherical, oblong, caudate, fusiform, and triangular, about one twelve hundredth of an inch in diameter.

In short the microscope gave evidence of a malignant type of growth, and to the naked eye the tumor presented the appearance of medullary cancer. Yet the history of the patient's illness, the

almost uniform and slow growth of the tumor during eight years, its external characteristics, and the absence of pain, would warrant a very different conclusion.

Case IV.—This and the next case I mean very briefly to allude to as they are still under treatment. The present case is that of a lad who was admitted to hospital on January 24, 1870. Ten days before admission he had a rigor followed soon after by pain, redness, and swelling in the left leg. He is a very delicate looking boy, but up to the day he took ill had been constantly at his work.

The disease was at first treated for erysipelas, from which acute periostitis and necrosis needs to be very carefully discriminated, as the only hope of saving the bone in the latter is by very early incisions.

When examined shortly after admission, the limb, from one extremity to the other, was occupied by a bag of fluctuating pus. There was great swelling about the ankle, and a superficial ulcer over each malleolus. A free incision was at once made, and matter, mixed with blood clots, evacuated to the extent of half-a-pint. On introducing a finger into the wound, it was found that the pus had been contained between the periosteum and the bone from which the former was detached, as far as the finger could reach, in the axis of the bone as well as all round it. The boy was much exhausted, and I felt certain the entire diaphysis had perished. He did not come under my care till he had been a week in hospital, and I then commenced antiseptic dressings. The disease, however, had largely implicated the ankle-joint, and it was showing symptoms of travelling upward to the knee. The discharge continued very profuse, and the pulse rose as high as 140. His condition had become desperate. As a last resort, and as affording the only prospect of saving his life, I determined, with the consent of my colleagues, to amputate on the 8th February. At this time he was so weak I feared he might not survive the shock of the operation; but it was evident, from the rapidity of his downward course, he could not survive many days if he were not relieved in some way. What I feared chiefly after operation was pyemia. I believe it is especially likely to occur in such cases as this one. For this reason, and believing the shock to be less, I determined to amputate through the joint, although the parts of which I had to form my anterior flap were œdematous. This was not only so, but

in forming the flap I cut through a small abscess. The operation was performed in the usual way under chloroform. Seven vessels were secured by carbolized catgut ligatures, including the popliteal vein, and the ends cut short. He rallied well after the operation, the pulse came down, and he expressed himself as being greatly relieved. The subsequent history is, that the anterior flap sloughed in that portion where the abscess and œdema were situated, the condyles were exposed, and the cartilage became slowly replaced by granulations. An abscess formed high up in the thigh, and his convalescence has been very tedious. I trust, however, now, that he is in a fair way to a good recovery. He has been up daily and out of doors. The amount of disease, considering that it had been of only three weeks' duration, which the amputated limb displayed was remarkable. The entire shaft and lower epiphysis were necrosed, and the superior epiphysis was beginning to separate. The periosteum, which in one place had formed some new bone, was completely detached throughout. It presented a thick velvety injected surface. The ankle-joint was completely disorganized, and the cartilages of incrustation were thrown off. A similar condition existed in all the other tarsal joints, and the bones of the tarsus were softened and inflamed. The tarso-metatarsal joints were not affected, and the disease had not extended so as to implicate the knee-joint.

Case V.—This man, although only twenty-seven, looked very much older. He had contracted syphilis nine years ago, and has suffered severely from its manifestations ever since. He was treated at different times by mercurials in many forms, and he has taken iodide of potassium in doses varying from five grains to sixty grains. He submitted to a protracted course of syphilization. He is marked all over with the scars of the syphilitic ulcerations, which have formed on different parts of the body. For the last two years the disease appears to have become localized in the left leg and foot, which is treble the normal size, covered with tubercular masses and large painful ulcers. The agony he has to endure caused him to have recourse to opium, and he admits taking four ounces of laudanum daily. His friends say he takes a good deal more. One of the toes has just dropped off, and the others seem on the point of doing the same thing. The poor man placed himself in hospital, under my care, and begged that the limb might be removed at any risk. The tibia had been the seat of antecedent disease, and there were small ulcers in different parts of the leg. The patient himself

was very urgent that the whole leg should be removed. I therefore determined to disarticulate, and performed the operation under chloroform, on February 12, 1870. I made the flaps as in the other cases, commencing nearly two inches below the line of the articulation. Both the anterior and posterior flaps were made rather longer than before. Notwithstanding this, the tendency to retract was very great, and the soft parts would have been pulled right away from the condyles had they not been restrained. It is mentioned by several writers as one of the great recommendations of this form of operation, that retraction of the flaps does not occur. In the present instance this objectionable tendency was present in a most aggravated form, and I have noticed it, though not to the same extent, in previous instances.

The main artery and vein, and other vessels were secured with carbolized catgut ligatures, the ends of which were cut short, and carbolized oil dressings were applied. I previously inserted thick catgut stitches in the flaps, which I find do not hold a sufficiently long time.

There was no shock, and no febrile reaction took place. The patient was doing in every way well, except that the edge of the anterior flap became gangrenous—just the edge—but more than sufficient to prevent union. The catgut sutures after a time gave way, and in the depths of a very unhealthy wound the popliteal artery was seen beating with all its force. This was a week after operation. Presently the situation of the vessel was covered by healthy granulations, and in another week the pulsations became weaker; but it was not for more than three weeks that they subsided so far as to dissipate all fear of secondary hemorrhage. Meanwhile, no trace of the catgut ligatures was seen. They seemed to have been covered over with the granulations, and to have become absorbed.

At the time I write the man is quite convalescent. There is still a considerable extent of granulating surface to heal, but this is closing, and when the stump is soundly healed, I have no doubt it will prove an excellent and useful one.

We may now proceed to consider the value of this operation of disarticulation at the knee-joint. We find very various opinions entertained about it. I believe that, like other things human, it is far from being perfect, but the weight of evidence is strongly, I think, in its favour. With very few exceptions, we may conclude that the mode of operation here described is that which will most generally be adopted. There is doubtless a latent dislike to bisect

a large articulation like the knee, conversant as we are with the very serious consequences which attend the most trivial injuries of it. As a matter of experience, however, the free opening of the joint in amputation is apparently followed by no special consequences such as, *a priori*, we might anticipate. Danger from this cause must be of very rare occurrence. The two forms of injury, in fact, belong to different categories.

Another objection, felt strongly by some, rests in the presence of the articular cartilages, which must needs, they say, die and exfoliate, causing delayed healing, tender painful stump, and endless trouble and danger. Mr. Butcher, for instance, says in his *Operative and Conservative Surgery*, "I have no doubt whatever that in all cases the cartilaginous surface of the femur should be removed." He recommends a curved section of the bone as giving as good a surface for support as where the cartilages remain untouched. Now after disarticulation one of two things will happen, either immediate union takes place, or some portion of the anterior flap may slough, and the cartilage of incrustation become more or less exposed. This latter is certainly the more frequent result. Indeed, Dr. Brinton considers sloughing of the margin of the flaps occurs to some extent in most cases. But supposing union do occur by the first intention, the cartilages do not exfoliate, and do not give any further trouble. In the case recorded by Mr. Lane, and in other cases, there was free motion both of the patella, and of the flap, over the subjacent surfaces. Immediate union took place without exfoliation of cartilage. In all three of my cases that survived, the patella remained moveable, and in Allen's case the flap was partially moveable also. In process of time the cartilage no doubt becomes altered and absorbed, as tissues always do for which there is no longer any use, and eventually the flap and the bone will become joined by some connecting medium, but meanwhile the articular cartilage affords a smooth desirable resting place for the flap. But suppose a less fortunate issue to the operation, suppose union do not take place, that there is retraction of the flaps in consequence, and exposure of the cartilage. In that case exfoliation will occur, but not for the lapse of two or three weeks, not in short until the slough have been cast off, and the stump may be supposed to be granulating healthily. In the interval I consider the cartilage has served a most valuable purpose. It is an unabsorbent protective to the end of the bone, shields it from the influence of the hurtful secretions of the wound, and by the time it is exfoliated

or disintegrated, the end of the bone is covered and protected by a granulating surface. This, in my mind, is a condition which contrasts most favourably with an operation, where other conditions being alike, the bone, in place of being thoroughly protected, is divided through its cancellated structure, whose patulous porous nature is just such as to invite the entrance of the noxious discharges of the wound. I am satisfied that this condition, other things being equal, is calculated to induce pyemic poisoning.

No advantage is derived from sawing off the condyles. When healthy they should most certainly be retained. The presence of the articular cartilage does not interfere with the healing of the wound, and if primary union do not take place the safety of the patient is better secured by the presence in the wound of a cartilage-covered bone, than by an exposed surface of cancellated tissue. Dr. Stephen Smith, the latest writer on the subject, adverting to this point, remarks that "the methods of Syme and Carden, involving excision of the condyles, are extremely objectionable, as well as entirely unnecessary. The great source of disappointment which I have met with is the want of sufficient vitality in the long flap. In every instance in which I performed the operation the edge of the flap sloughed more or less. In all the cases, save that of Allen, there were doubtless constitutional causes present sufficient to account for the gangrene, still, as shown in that instance, there will be, under even favourable circumstances, an almost fatal liability to sloughing. This is due, of course, to the length, tenuity, and the inadequate arterial supply of the anterior flap.

Possibly greater care in guarding against the hemorrhage of reaction, in dressing the stump, or the abstinence from dressings of all kinds, care, in short, to prevent any pressure upon the flap either from within or from without might serve to avert this unfortunate contingency. As helping towards the same end, Mr. Pollock has pointed out how important it is not to cut a single line higher up between the anterior and posterior flaps than what is absolutely necessary, and that the incision should not commence above the lower edge of the condyles of the femur. I was myself so much impressed with the necessity of this precaution that I invariably began my incision on a plane very much lower than that indicated by Mr. Pollock, namely, nearly one inch below the margin of the head of the tibia. It must also be recollected that the broader the base of the flap is made the less the risk of gangrene will prove. In judging of the results of the operations I have performed, it must

be conceded that, with the exception of Allen, it would be difficult to find more unfavourable cases.

Harbinson was suffering from long continued disease of the tibia, in which an abscess, subsequently bursting into the joint, had formed. He refused operative interference for a long time, and claimed it when too late. Pyemia, of a very acute type, though it had not declared itself at the moment of amputation, shortly after set in, and he was soon carried off. The operation did not appear in any way to shorten his career.

Acheson, a sickly strumous lad, was attacked with acute necrosis of the entire shaft of the tibia, and destructive inflammation of all the tarsal bones. An amount of damage, such as is rarely met with, took place in the short term of three weeks. When the operation was proposed he appeared to be almost in a dying state. The nurse, a most experienced woman, said "that boy, I fear, will not come back to the ward alive." Yet, though the prospects seemed so hopeless, there was not only no shock, but almost immediate relief was experienced, and I expect he will yet do well. Crawford was but little better, worn out, as he was, by nine years' constant suffering, a confirmed laudanum drinker, the tissues of the limb diseased, the muscles only to be compared to living adipocere. What was to be expected in such a case after a capital operation? Yet here, too, there was complete absence of shock. In spite of the partial gangrene, in spite of the ominous beating of the popliteal artery, he was able to sit up daily a fortnight after the operation. He is now progressing to complete recovery, and will in time possess an admirable stump. He has since left the hospital.

A sufficient interval has now elapsed to show what Allen's stump is like. It was photographed some four months after the operation, and no graphic power is needed to describe its great length and consequent power, its broad rounded extremity, covered with thick integuments, and well calculated to sustain pressure, while the line of cicatrix is quite posterior and out of the way.

Mr. Pollock, with good reason, speaks of the stump resulting after amputation at the joint, "as most satisfactory, and the adaptation of an artificial leg as most easy." Dr. Brinton goes further when he states that "patients, surgeons, and surgical mechanicians are agreed, their testimony is nearly unanimous as to the serviceability of the stump, and its fitness for the adaptation of an artificial limb."

One remarkable instance in support of these views, I may perhaps be permitted to quote:—"E. K. P., aged twenty-one,

received a shell wound of the leg at the battle of Cedar Mountains, U.S., Aug. 9th, 1862. Primary amputation at the knee was performed by Surgeon Bentley, U.S. Volunteers. The articular surface of the condyles was sawn off, the patella was left. The patient recovered without accident in four months. He has since been engaged in four battles, and on one occasion was captured, and obliged to walk fifty-four miles between sunrise and nine o'clock p.m. The stump is painless and well formed, and he can walk, or dance, mount his horse and ride, with perfect facility."

The rate of mortality attendant on amputation through the knee, as compared with amputation of the thigh, is, I think, somewhat difficult to arrive at. I question if there be a sufficient amount of material wherewith to form a correct estimate. The largest number of cases is contained in the invaluable reports issued by the United States' Surgeon-General. In Circular No. VI. Surgeon Otis records 132 cases of knee-joint amputations up to 1864, with a death-rate of 48 per cent.

In Dr. Brinton's most interesting communication, already more than once referred to, that surgeon relates that further returns have been made to the Surgeon-General's office, which raise the number of knee-joint amputations performed to 211. Of these 106, or 50·2 per cent., proved fatal. Many of these additional examples, Surgeon Otis explains, are cases of primary amputation, collected from special reports, which died before reaching hospital.

Mr. Pollock has collected forty-eight cases from various sources, operated upon in England, with thirty-six recoveries and twelve deaths, or thirty-three and a third per cent.

Dr. Markoe gives the following table, showing the results in the aggregate of American and foreign cases of amputation at the knee-joint:—

	Cases	Recoveries	Deaths	Mortality per centum
Primary amputations after accident, -	59	34	25	42·37
Secondary amputations after accident, -	37	23	14	37·83
Secondary amputations for disease, -	62	48	14	22·58
Cause and period of disease undetermined,	6	6	—	—
Total, - -	164	111	53	35·31

Mr. Pollock has had himself nine cases and one death. Of my four cases one only proved fatal, besides the Carden's amputation.

So far, too, as the comparison has gone, it would appear from the cases collected by Dr. Brinton that the ratio of deaths is somewhat greater when the condyles have been removed than when they are left intact, and on reading over these cases one cannot fail to be struck with the relative frequency of death from pyemia amongst the former. Against this, however, may fairly be quoted Mr. Carden's remarkable success of thirty-one cases and five deaths.

Undoubtedly, the death-rate in this operation will contrast favourably with amputation of the thigh, forming the estimate from cases similarly circumstanced.

In the late American war, Surgeon Otis reports 1,597 completed thigh amputations, and 1,029 deaths, or 64 per cent., and in civil practice the mortality attendant on primary amputation of the thigh cannot be much less than 60 per cent. Fifty per cent., or, according to others less, is the ratio for amputation through the knee. If Dr. Markoe's rate of mortality of 22 per cent. for amputations through the knee-joint for disease be correct, this also will bear favourable comparison with pathological amputations of the thigh in civil practice.

Dr. Hamilton, in his *Military Surgery*, considers that disarticulation at the knee has proved more successful and more safe than amputation either immediately above or immediately below the joint. And Dr. Stephen Smith thinks it established that this operation is less fatal than amputation in the upper third of the leg.

I do not agree with these authorities. I would not feel myself justified in amputating at the joint, if it were practicable to do so at any point lower down. I think disarticulation should never from choice be substituted for amputation of the leg. The cases, then, to which it is applicable must prove comparatively rare. They are injuries to the knee-joint, not involving the femur, or only involving it to a slight degree, extensive disease, or severe compound fracture, involving the whole of the tibia, but without great destruction of the soft parts over the front of the limb.

In very many cases where amputation of the leg is inadmissible, the state of the parts will be such as to necessitate amputation of the thigh. Possibly this may be one of the chief reasons why amputation through the knee-joint has for lengthened periods been completely abandoned, to be from time to time revived as surgeons happen to meet with cases which appear eminently suitable for it.

Since writing the foregoing paper I have again performed

disarticulation at the knee-joint with every prospect of a most satisfactory issue. It was in the case of a little girl of thirteen years old. Six months ago, acute periostitis and necrosis of the tibia took place, and a sequestrum, comprising half the shaft of the tibia, was afterwards removed. An interval of five months passed, and no new bone was deposited. The limb became greatly deformed, and was quite useless. Finding that there was no hope of preserving a member of any use to the patient, and finding that the knee-joint was becoming involved, that there was free lateral motion in it, and some grating between the head of the tibia and the femur, I considered amputation not only expedient but necessary. Her general health, too, was beginning to give way, and the wound made to remove the necrosed tibia had never healed. The little patient being chloroformed, I first made a posterior cutaneous flap, about three inches long, and dissected it up to the level of the articulation, including in it all the subcutaneous tissues down to the muscles. I then made an anterior flap somewhat longer, opened the joint, and cut the rest of the parts by a circular sweep of the knife. The flaps were carefully formed with a scalpel, cutting at right angles to the surface, and their edges were thus quite thick. The vessels and nerves were divided transversely. Catgut ligatures, and Mr. Lister's lac dressings were employed. Although the flaps were nearly of equal length, the anterior one folded completely round the end of the femur which was not interfered with, and met the retracted posterior one an inch and a half above the end of the condyles. The cartilage on the femur was sound, that on the tibia completely disorganised.

The total absence of shock after the operation was very evident when I visited the child in the evening. Her face was perfectly tranquil, she was not suffering pain, and her appearance betrayed not the slightest indication that a serious operation, or indeed any operation at all, had been performed upon her. Ten days have now passed, and the child has continued quite well. The wound is healing admirably, and there has been no sloughing or trouble of any kind.

ART. XII.—*Some Practical Remarks upon the Treatment of Syphilitic Disease.* By JOHN K. BARTON, M.D., (Dub.) F.R.C.S., &c.; Surgeon to the Adelaide Hospital; Lecturer on Surgery in the Ledwich School of Medicine, &c., &c.

THE principles upon which the treatment of syphilitic disease should be conducted, are sufficiently simple, yet, if I may judge

from cases which have come under my own observation, they are not as generally recognized, or as universally acted upon, as their importance demands. In these principles I do not include the use of any particular therapeutic agent, but simply the general rules which a knowledge of the natural course of the disease, and of the effects produced by certain medicines on the symptoms, have enabled us to arrive at; these form a safe groundwork upon which to determine the line of special treatment for adoption in any particular case. In the following pages I purpose drawing the readers' attention to these principles, and illustrating their practical utility by cases, in some of which the same remedy (mercury), will be seen to have produced, when given upon a *wrong* principle, aggravation of the symptoms, with great deterioration of the general health, and subsequently, when given upon *right* principles, marked improvement of the general health, and a disappearance of the symptoms.

Beforing entering upon this subject, I am compelled by some observations[a] made in a recent communication in the pages of this Journal, briefly to refer to the well worn controversy concerning the distinction which exists between the three forms of venereal disease. The conclusion sought to be drawn from some of the cases referred to in this communication is such, as, if adopted, would throw us back into the uncertainty and confusion from which, thanks to the labours of many independent observers at home and abroad, we have recently emerged. The conclusion to which I refer is thus expressed—"On referring to the accompanying tabulations, it will be seen that, as a rule, all sores, whether soft or hard, were followed by constitutional signs."[b]—Upon referring to the tables as directed we find a certain number of cases in which it is stated that characteristic soft sores existed at the time of admission, and in which subsequently syphilitic symptoms became developed.

Admitting the facts to be as stated, and admitting also, which is not, however, at all proved, that in some at least of these cases the syphilitic symptoms which appeared, were not the return of old symptoms, but the first sign of constitutional infection; still the question remains, what proof is there to show that these sores were the initial lesion of the syphilitic symptoms which followed? There is no proof, for there is no mention made of any examination with

[a] Clinical Review of Cases under Treatment at the Westmoreland Lock Hospital. By John Morgan, F.R.C.S.I., &c. Dub. Quart. Jour., Nov., 1869.

[b] Dub. Quart. Jour., Nov., 1869, page 508.

the speculum to ascertain the possible existence of other sores; nor is there even any careful account of the time which elapsed between the development of the sores, and that of the syphilitic symptoms.

In the absence of all such data there are three ways of explaining the facts recorded in the tables, and it will be admitted we are bound, in endeavouring to explain any phenomena, to adopt the most reasonable and likely, not the most unreasonable and unlikely solution of the difficulty. The *first* explanation which suggests itself at once is, that these "soft sores" had nothing whatever to say to the syphilitic symptoms, being another disease, which prostitutes in the constant habit of promiscuous intercourse, were, to say the least of it, not unlikely to contract, in addition to a syphilitic sore, which they either had not noticed, or, having noticed, considered of no consequence; but which trifling sore was really the initial lesion of the subsequent symptoms. If this explanation be objected to in any instance, the facts before us admit of a second explanation equally satisfactory and probable, and involving no difficulty, which is, that in some cases the "soft sores" were true syphilitic chancres, in which the sign of induration was wanting at the time of observation, for it is well known that *induration*, however valuable a sign when present, is not to be looked upon as absolutely necessary to the syphilitic chancre; and indeed for this reason the names of hard and soft as applied to the infecting and non-infecting sores are very objectionable; but in the tables in which these cases are given, the sores are simply described as "soft sores," or a "number of soft sores," the other signs of *surface, secretion*, &c., being altogether omitted. It seems probable that in some of the cases the former and in others the latter explanation was the true one.

The author of these tables, however, adopts a very different explanation of the simple phenomena there stated, which is—that these "numerous soft sores" were examples of what is commonly called the local venereal sore or chancroid, which, indeed, is very likely; but—strange conclusion—that the secondary syphilitic symptoms followed from them. I have shown that there is not the slightest necessity for any such conclusion, as the cases admit of ready explanation without it; but if adopted we must at once pronounce all the careful and laborious researches which have been made upon this subject, since Bassereau, in 1835, began his observations, up to the present time, to be altogether wrong, and this rather than admit that a certain number of women in the nightly habit of prostitution might have two kinds of venereal disease at the same time.

I leave the reader to adopt whichever of these explanations he thinks most reasonable.

Syphilis in any of its stages may present difficulties of *diagnosis*, but more frequently in its later stages is this the case. The plan generally taken in such cases is to make a minute inquiry into the antecedents of the patient, and the diagnosis is formed very much upon the information thus acquired. This appears to me to be a grave mistake, and one which very oftens leads the surgeon into error. To found a diagnosis in any case upon the history of his symptoms, as given by the patient, is always dangerous, but much more so in syphilis than in other diseases, for so chronic is the disease and so various the symptoms it produces, that the patient may readily deceive himself and the surgeon consequently who trusts to what he tells him.

Under any circumstances of difficult diagnosis, I am convinced that our efforts should be directed almost exclusively to a careful, and, if necessary, a repeated examination of all the signs, which the skin, nails, hair, mucus-membrane of the mouth and fauces, &c., will present, rather than rest any weight upon the history given by the patients. For obvious reasons some patients will deny the possibility of their having contracted[a] syphilis, while others will unintentionally mislead in the opposite direction. Several cases have come under my observation in which the patients have given a history of primary sores, followed by an eruption, which, upon closer investigation, turned out to be a rash produced by copaiba, or an ordinary

[a] A brewer's man, aged twenty-seven, married, and accustomed to drink freely of beer, presented himself at the Adelaide Hospital, upon the 25th of September, 1869. He had a sore upon the penis, partly upon the reflected prepuce, partly on the glands, red, smooth, excavated and indurated, glands in groin enlarged and hard, complained of weary pains in his bones, worse at night; post cervical glands enlarged, and hair commencing to fall; no eruption on skin, nor any sore throat. When asked as to when he contracted the sore he said he first perceived it about three or four weeks previously, but positively denied ever having had connexion with any woman but his wife, who was the mother of several healthy children. Totally disregarding these statements and relying upon the distinctive features of the disease, which were well marked, I made the diagnosis. "Syphilis in the stage of premonitary symptoms before the ushering in of secondary lesions," and to confirm this took the patient into the hospital, where he was kept in bed and given a diaphoretic mixture; within a week a well marked papular eruption began to make its appearance, accompanied by sore throat, and simultaneously the fever and pains were relieved. The future progress of the case, up to the present time, amply confirmed the original diagnosis; but to the last, although often questioned, the patient denied the possibility of his having any venereal disease; but when in his hearing, I urged upon the students the necessity of making a diagnosis in such cases, quite irrespective of the history given by the patient, he was observed to relapse into a broad grin.

skin affection (scabies, excema, or purpura). On account of this double source of error, I view with suspicion any diagnosis founded upon the previous history. *The signs which we ourselves have an opportunity of observing, are the only reliable grounds of a diagnosis in any difficult case,*[a] and by carefully noticing them we will always be able to come to a definite conclusion, and either confidently enter upon treatment, or on the other hand dismiss the subject of syphilis from our patient's mind.

I have elsewhere[b] stated my views of the *treatment* of syphilis; these views have been confirmed by more recent experience, while at the same time I am convinced that in order to make a change in the old method of giving mercury rapidly and in large quantities for syphilis, it is necessary to insist upon the principles which should guide us.

The first general rule or principle of treatment which I would suggest, is, that our *object* should be to remove all existing symptoms as completely as possible, *not* to drive out the virus from the system; the virus we only know by its effects, and there is no proof whatever that the virus is directly influenced by any treatment. There is, however, very good reason to believe that the virus is in a large number of cases really eliminated from the system, after the lapse of a considerable interval of time, during which its presence in lessening activity is seen, and there can be little doubt that by removing the symptoms we indirectly aid in this elimination, so that while we confine our treatment to the removal of existing symptoms, we are accomplishing, as far as treatment can, the purification of the system; but this must not be the object we propose to ourselves or our patients. When the symptoms have totally disappeared we have no further safe guide; but might, with the theoretic object of freeing the system from the virus, persevere indefinitely to the destruction of the patient's health, and after all be disappointed by finding a new batch of symptoms appearing when his cure was pronounced complete.

Experience, I think, however, shows that while our object must be simply the removal of the symptoms, yet that in order to do this satisfactorily, that is, to prevent, as far as possible, *a return* of the symptoms, we must persevere with treatment for some weeks after

[a] I find other surgeons have adopted a similar rule. Mr. C. Forster says—"For many years I have enjoined my reporters only to note down what they see."—Guy's Hospital Reports, 1870, p. 162.

[b] Syphilis, &c., Chap. ix., page 200, &c.

all signs of the disease have disappeared; this is not necessary in all cases, but in the majority, especially such as are in the second stage, this is a wise and useful precaution.

The second general principle is, that whatever therapeutic means we employ should be used in a gradual and gentle manner, in contra-distinction to the active and energetic use of the same drugs. The reason of this is, that syphilis is essentially a chronic disease; its symptoms are gradually developed, and continue for a length of time, gradually disappear, and are followed after an interval by another set of symptoms which pursue a similar course. That a remedy which acts energetically and rapidly upon the system, cannot successfully combat such a disease, requires no argument, and yet such a method of treatment is, in fact, very generally adopted, with the result, as some of the following cases will show, of rendering the disease more intractable, while the health of the patients becomes much deteriorated. The rule that an acute disease requires an active remedy, while a chronic disease will only yield to a slow remedy, is generally accepted; there is no disease with which I am acquainted, in the treatment of which this rule should be more carefully borne in mind than in that of syphilis, not only from the essentially chronic character of the disease, but from the fact of the medicine, which possesses the greatest power over its symptoms, and is generally employed in its treatment, being *mercury*, which metal when introduced into the system in too energetic and rapid a manner is capable of doing as much harm, as on the other hand, when used in a cautious and discriminating manner, it is powerful for good. There can, I conceive, be no doubt that by means of mercury the effects of the syphilitic poison in the system may be counteracted to a very great extent, the duration and severity of the disease mitigated, and life saved when vital organs are attacked, and yet the non-mercurialist may boast that his cases on the whole get on quite as well, and some of them very much better than those under the care of practitioners who employ mercury. In an average of a large number of cases treated with and without mercury, I believe there would be found no great difference either in duration or severity of symptoms, and the reason I am convinced is, that on the mercurial side the cases which are improved and relieved by mercury judiciously administered are counteracted by those in which it has been used in too active and energetic a manner, so that if this second general principle is not attended to, it would be much better not to give mercury at all.

Salivation should never be induced in the treatment of syphilis; all the good effects of the drug can be obtained without this, the poisonous effect of the metal upon the system. Not only is the patient injured by the depressing effects of mercury when pushed rapidly on to salivation, but the syphilitic symptoms remain unaffected by this active use of the drug. So that it is always a most unfortunate circumstance should a patient become salivated, and one which we should guard against by, in the first place, using mercury, especially at first, very cautiously, and in small quantities, and also by examining our patients frequently, for some are very susceptible. The best means of giving mercury so as to fulfil the indication of introducing it gradually into the system, and avoiding salivation, is, I believe, by means of inunction; ten or fifteen grains of mercurial ointment carefully rubbed in daily will do more for the relief of the symptoms, than could be credited by those who are accustomed to give it freely by the mouth. Inunction is much the best means to employ in the early stages of the disease, but in the later stages, very often it is necessary to give mercury in combination with iodide of potassium, and then either the biniodide or bichloride may be given in doses of the twelfth of a grain, and the metal will then act in the gradual manner required. The following cases prove the importance of due attention being given to this principle of the gradual introduction of the remedy by which we propose to influence the symptoms of syphilis.

Case I.—*Syphilis in its Second Stage, treated at first by the rapid introduction of Mercury into the system, which, while injuring the patient's health, failed to remove the symptoms—Subsequent gradual treatment, followed by complete removal of the symptoms and restoration of the patient's health.*

J. R., a clerk, came under my observation upon the 3rd of December, 1869. He was then profusely salivated; bloody saliva flowed constantly from his mouth night and day; he could with difficulty swallow even drinks. He was bathed constantly in a cold sweat, his pulse being rapid and weak. The syphilitic symptoms were a papular eruption, well marked on the face, arms, and neck, enlarged cervical glands, pains along both tibiæ, which were worse at night.

He had contracted the disease in August, and the eruption had appeared, he stated, during the following month. He was at this time in the country. The surgeon whom he first consulted gave

him "potash," and he thinks a little mercury, when the eruption and other symptoms became somewhat better. He came to Dublin about business, and then consulted a medical man, who prescribed pills for him, one to be taken three times a day, and after a few days fearing one of his eyes was becoming inflamed, gave one every four hours. At the end of the week salivation was established, with all its attendant evils, while the eruption and other symptoms remained the same as they were before this active treatment was commenced.

When this patient came under my care, the treatment was necessarily entirely directed to the relief of the salivation. Four grains of quinine was given every four hours, while a chlorate of potash gargle was constantly used to wash the mouth; fluid nourishment was given every four hours. In about a fortnight he was recovered, so far as to eat solid food; he then left for the country. I saw him again early in January; he had then quite recovered from the debilitating effects of the salivation, and appeared in good general health. *The syphilitic symptoms remained as well marked as before.* I now considered him in a suitable state for treatment, and commenced at once with the inunction of half a drachm of mercurial ointment once daily, a mixture containing three grains of sulphate of quinine in each dose being given twice a day. After a fortnight's steady perseverance in this treatment the eruption was manifestly decreasing; the inunction was then reduced to fifteen grains daily. Upon the 8th of February, after a month's treatment, the following note was taken:—"Papules scarcely perceptible on the forehead, where they had been very marked; eruption on arms and back nearly disappeared; the papules which remains are scaly; slight enlargement of cervical glands still on one side; no pains or other symptoms; general health very good; has not been a day from business since he returned to town; no symptoms of salivation." By the end of March all the symptoms had completely disappeared, and the treatment was discontinued.

Case II. (Reported by Mr. Arthur Ringland).—*Syphilis in its Tertiary Stage—Salivation produced upon five different occasions without relief to symptoms—Complete removal of symptoms and restoration of general health by gradual treatment.*

A. P., a widow, about thirty years of age, was admitted under the care of Dr. Barton, into the Adelaide Hospital, upon the 12th November, 1869; her condition was then as follows:—An eruption

of both dry and ulcerating tubercles was scattered over the body and limbs; one ulcerating patch upon the back and another near the left wrist-joint, were each as large as a five shilling piece; the surfaces of these ulcers were covered with a dirty semipurulent discharge, their edges red and everted. Several large white cicatrices marked the situations where similar ulcers had healed. Severe pains were felt in both arms, legs, and head; in the latter the intensity was such as to resemble that of severe neuralgia; all these pains were worse at night; the pains along the tibiæ and about the ankles and knees were much increased by standing, so that the patient could scarcely bear to put her feet to the ground. Her hair had fallen to a great extent, and was still falling.

Previous History.—Two years before, her husband had sores on his lip, and she got a similar sore on her upper lip from him; this sore ulcerated, and was followed by a cutaneous eruption, which, commencing on the face and head, spread over the entire body. The throat became very sore about the same time; this was followed by inflammation of both eyes, from the effects of which her sight remains impaired to the present time. When the eruption first appeared she was placed under the influence of mercury, and salivated. She was again salivated, when her eyes became inflamed; when these got better the eruption, pains, sore throat, &c., remained; for these she was given mercury again, and subsequently, in the hands of other medical men, she was salivated twice, so that altogether she has been salivated, during the past two years, five times, and yet she states the symptoms of the disease are as bad now as ever. The patient was emaciated, and had all the appearance of one worn out with loss of sleep, pain, and depraved digestion, and expressed herself as almost despairing of relief.

As the periosteal pains were the most distressing symptoms, and those which deprived her of rest and kept up the irritative fever, her treatment at first consisted in eight grain doses of iodide of potassium, four times in the twenty-four hours, with Dover's powder at bedtime. In about three days there was a decided relief from pain; the mixture was then changed to one containing the twelfth of a grain of biniodide of mercury, and five grains of iodide of potassium in each ounce of decoction of cinchona bark; of this she took one ounce three times a day. She was placed on meat diet with some wine; ordered to take a hot air bath twice a week, and to have the ulcers fumigated with calomel.

Under this treatment the patient steadily improved in every

respect, her general health being restored *pari-passu* with the disappearance of all the syphilitic symptoms. The ulcers soon put on a healthy appearance, but their cicatrization was very slow. After all other symptoms had quite disappeared the ulcer on the back and that on the left wrist still remained. After about five weeks of treatment the patient's improvement was so decided as to attract the attention of all who had seen her when first admitted, and to elicit from herself the remark that she never expected to be so well. About the middle of December, this patient was attacked with shivering and pain in the right side, which proved to be due to diaphragmatic pleuritis, which soon spread to the lungs, and her life was in imminent danger. The right lung becoming solid without at the same time any expectoration, with the assistance of my colleague, Dr. Little, this serious complication was, however, overcome, and the patient made a good recovery. She left for the Convalescent Home, Stillorgan, upon the 21st of January, the syphilitic symptoms having entirely disappeared, and her general health being better than it had been since she contracted the sore upon her lip two years previously.[a]

Case III. (Reported by Mr. Arthur Ringland).—*Tertiary Signs two years and a half after Infection—Salivation without removal of Symptoms—Great General Cachexia—Symptoms removed, and General Health restored by Treatment without Salivation.*

William De C., nineteen years of age, was admitted into hospital upon the 18th December, 1869. He presented the following group of symptoms:—Tonsils and palate bore the cicatrices of former ulceration, and were still red and inflamed looking. Spots of a dark coppery colour were scattered over the face; several cicatrices of rupia existed over the shoulders and back.

Upon the upper and inner part of both legs, near the knees, existed two large ulcers, the one upon the right leg being the largest, and being about the size of a five shilling piece; the edges of these ulcers were not undermined, but the skin around them was of a dark coppery hue; the surfaces were covered with a dirty looking greyish exudation. He complained of nocturnal pains in his

[a] This patient presented herself again at the Hospital in March last, and exhibited two ulcers about the size of a shilling each upon the scalp. Her health was failing; she had been leading a drunken and irregular life. She was admitted, but when she came to the Hospital in the evening she was so drunk as to make it necessary to cancel her admission.

head and legs, so severe as to deprive him altogether of sleep. Upon the top of the glans penis existed the cicatrix of an old sore; the glands in both groins were enlarged and indurated; his tongue was white; there was complete loss of appetite; he was emaciated, and his skin had a yellowish dry look.

Previous History.—About two years and a half before his admission to hospital, he first noticed a single sore upon the glans penis, the mark of which still exists; this was followed shortly by an eruption upon the forehead. He then sought for medical advice; the surgeon whom he consulted cauterized the sore, and subsequently, the eruption having spread all over his body, placed him under the influence of mercury. He became severely salivated, but states that the eruption continued without much change for four months. He now began to drink rather freely, and became infected with gonorrhea; this was followed by his present symptoms of great general debility, and by the appearance of the syphilitic symptoms which he presented upon admission.

The *treatment* adopted was, internally, eight grain doses of iodide of potassium, with the one-sixteenth of a grain of biniodide of mercury, in an ounce of decoction of cinchona bark, three times a day, a mercurial vapour bath every second day, ten grains of calomel being used each time, the ulcers dressed with charpie soaked in black wash; full diet. For several nights after his admission scarcely any sleep was obtained, but he expressed himself always relieved by the baths, and the ulcers began at once to show a more healthy edge and surface. A steady perseverance in the treatment was, however, soon followed by a gradual improvement in all his symptoms; the eruption disappeared; the ulcers healed; the nocturnal pains subsided; and at the same time natural sleep and appetite returned; not the slightest symptoms of salivation appeared. The treatment was continued with but slight alteration until the beginning of February, when being free from all signs of disease, and feeling well, he left the hospital. He has been seen occasionally since that time, and he stated he was free from any symptom such as he had suffered from when admitted.

Case IV. (Reported by Mr. Samuel Knaggs).—*Tertiary Stage—Cerebral Symptoms—Acute Head-ache—Loss of Sight—Loss of Memory—Complete Recovery.*

J. C., a respectable married woman, forty-one years of age, applied at the Adelaide Hospital in October, 1868. She then had

a gumatous tumor on the thigh; four years previously she had contracted syphilis from her husband, and had ever since been suffering from various symptoms of the disease. She had been for several months in Dr. Stevens' Hospital; when better she went out, but subsequently, when the symptoms returned, she placed herself under Mr. Hamilton's care, who salivated her; still her symptoms continued with short intervals of apparent health. She did not come into hospital at this time, but was treated as an out-patient. Upon January the 4th, 1869, she presented herself with a fresh train of symptoms, and was then admitted. She now complained of intense pain of the head, like a tight band; she compared it herself to a feeling as if a red-hot iron circle was tight round her head; both eye-balls were the seat of acute pain; the sight was very defective; flashes of light and *muscæ volitantes* were constantly before her; light could not be endured. Her memory appears to have been defective previously, but it was at this time much worse; recent occurrences seemed to her to have happened "centuries ago;" cannot remember anything; and says she has "spiritual and temporal delusions." The intense pain continued for more than a week after she was admitted, although every expedient, including large doses of iodide of potassium, was used to relieve her; any effort of mind, such even as the attempt to answer a question, increased the pain. Blisters were applied at the back of the neck, and behind the ears, and the twelfth of a grain of biniodide of mercury, with eight grains of iodide of potassium was taken four times daily.

A slow but steady improvement began after this treatment had been in force about two days—first, the pain began to abate in intensity, then the dread of light diminished, then the flashes and muscæ disappeared, and slowly the memory and capacity of mind returned. She remained about two months in hospital, during the whole of which time she continued taking the medicine. She then went to the Convalescent Home for about six weeks, and took the same medicine at intervals, a week being allowed to pass between each repetition of the bottle; all tendency to a return of her symptoms seeming then to have disappeared, at the end of about three months she discontinued the medicine. I have seen her within the last few weeks, and she tells me she has remained perfectly free from any symptoms of her disease whatever, and is able to support herself, filling the situation of housekeeper, but her memory is treacherous and defective.

Those who oppose all mercurial treatment, would find in the early history of each of these cases, ample grounds for the support of their views. We can, I think, hardly doubt that had these patients been treated upon a non-mercurial plan, or not treated at all except by hygienic rules, that they would have suffered less, and that the disease would have run a milder course than was the case under the treatment to which they were subjected. The conclusion, however, which the non-mercurialist would draw, viz.:—that mercury should not be given at all in such cases—does not necessarily follow, for I contend that the evident failure of mercury in these cases, was due, *not* to the wrong remedy being employed, *but* to the right remedy being wrongly employed, and the proof of this is furnished by the subsequent course which each of these same cases ran, where mercury was properly employed.

In the first case, the papular eruption which had continued, in spite of an active salivation, steadily declined, and disappeared under the inunction of fifteen grains of mercurial ointment daily, while the health remained perfect. In the second, an improvement such as caused the patient to say that "she never believed she would be so well," followed the use of the one-sixteenth of a grain of biniodide of mercury, with iodide of potassium, when five salivations and abundance of iodide of potassium alone had entirely failed to give relief.

In the third case, six weeks of the same treatment with mercurial fumigations to the ulcers, sent the patient out of hospital quite free from any symptoms; and in the fourth, in which serious head symptoms existed, similar treatment with counter irritation succeeded, not only in restoring the patient, but having been persevered on for three months, relieved her so much that she remains well after a year has elapsed, having previously been a constant sufferer, although more than once treated by mercury pushed to salivation. I do *not* claim for the mild or gradual method of mercurial treatment which I advocate, the power of eliminating the poison so completely from the system, that no future symptoms will appear, for I have found that patients treated according to the plan I recommend, will frequently show slight symptoms for a year or more. What I do claim for the plan is, that if employed in the early stage of infection, it will remove the symptoms completely, while the patient's health will at the same time improve, and that it will, if pursued for a sufficient length of time, render the future symptoms much milder and of shorter duration than they would otherwise have been.

If the disease is allowed to run its own course, or even if the patient is treated with active mercurialization, the mild mercurial plan cannot be so successfully employed subsequently, but still it remains the best treatment that can be adopted; it will still be able, in most cases, to restore the patient for a time at least to health, and to render the future recurrence of symptoms milder in their character. The methods which I have found the best in carrying out the mild mercurial treatment are—in the early stages, the daily inunction of from fifteen to thirty grains of mercurial ointment, and, in the later stage, either the inunction of ten grains of the ointment, or the mixture of the biniodide of mercury and iodide of potassium, as employed in the cases I have related; with either I occasionally use fumigation, either general or local, according to the nature of the case.

In a certain class of cases, of which the following is an example, fumigation is of much service:—

Case V.—*Tertiary Syphilis—Extensive Ulcers of Integument with Sarcocele—Treatment by Mercurial Vapour Bath.*

W. C., forty years of age, a servant, was admitted into hospital, on the 14th December, 1869. About nine years previously he had contracted a sore which was followed by an eruption on his skin, since which he had been almost constantly suffering from some symptoms of syphilis. His general health was much deteriorated. The left arm from the elbow to the wrist was the seat of several jagged ulcers, oval in shape, about the size of a five shilling piece each, their surfaces were covered with a fetid unhealthy pus, their edges raised and livid. Over the skin upon other parts of the body were scattered the indelible cicatrices of ulcerated tubercles. The right testicle was swollen to about six times its natural size, being heavy, firm, and painless; the left was atrophied.

Pains along the long bones were felt at night. A few indurated glands were to be found in the groin and in the neck.

The treatment which in this case was successful in relieving the pains, healing the ulcers, and restoring the general health in five weeks, was the calomel bath every second day, assisted by eight grain doses of iodide of potassium, in cinchona bark three times a day. No impression was, however, made upon the sarcocele, which remained the same, when the patient left the hospital well in all other respects.

In concluding these remarks, I would ask—How are the conflicting statements of those who advocate, and of those who denounce the use of mercury in syphilis to be accounted for? unless we admit that this medicine may be either useful or injurious according to the manner in which it is used. And again—Why has the external use of mercury been advocated by the most practical and able surgeons as the safest and surest method of treating syphilis in preference to the easier and readier method of internal administration? unless that it was found by experience that the symptoms yielded more completely and satisfactorily, while the general health remained unaffected by the remedy, when employed in this manner. The *gentle* and *gradual* action of mercury was thus gained, while its energetic and poisonous effects were avoided.

ART. XIII.—*On Extraction of Cataract by von Graefe's Peripheral Linear Section.* By HENRY WILSON, F.R.C.S., M.R.I.A.; Assistant Surgeon to St. Mark's Ophthalmic Hospital; late Teacher of Ophthalmoscopy at the Richmond Hospital, &c., &c.

THE subject of cataract has attracted a very large amount of attention from the earliest times, and if we consider how frequently this disease is the cause of blindness, and reflect on the difficulties encountered in its removal, we need not wonder at the very great interest attaching to it, nor at the largely increased amount of consideration and research devoted of late years to the means of its cure. Notwithstanding the ignorance or uncertainty of the ancients as to the true nature and seat of the disease, as is amply testified to both by their writings and nomenclature, many valuable instruments and operations were devised by them for its cure, some of which are retained even still. The needle was employed probably before the Christian era, and in its various modified forms still holds its ground, and is largely made use of; so long ago as the eighth or ninth century a hollow needle for the removal of cataract by suction was in use in Persia, and has been described and figured by Jesu Hali (Isa Ben Ali), a Christian oculist of great repute, living in Bagdad, in the ninth century.

Although we can thus trace back the needle to remote antiquity, the period when the knife, and with it the operation of extraction, was first employed is involved in obscurity. A passage in Pliny

has long led to the belief that extraction was practised in or before his time. The learned Professor Hirsch, of Berlin, has, however, lately brought his profound medico-philological knowledge to bear on the matter, and has given the weight of his authority against this supposition. He adduces evidence to prove that the word *squama* made use of by Pliny does not denote cataract at all, but corresponds to the Greek λεπις, a scale or splinter. A passage from Rhazes (Abu Bekr El-Razi), the Arab Galen of his time (ninth century), has been largely quoted to show that Latyrion, a name which is known only through this one passage, practised extraction. Dr. Hirsch is of opinion that this text does not bear the meaning thus attached to it, and says the assertions made of late years respecting the claims of Latyrion and Antyllus to this operation rest on gratuitous assumption, and copying erroneous quotations without reference to the original. He also remarks that the term *cataract* does not belong to remote antiquity, but that the first mention of it he has found occurs in Platearius a physician living in the twelfth or thirteenth century. Probably one of the first authentic records of extraction is that alluded to by Himly when he mentions that Freytag a surgeon in Zurich removed the cataract through a corneal flap wound in 1694; but so far as I can learn extraction was not practised until the beginning of the last century when the accidental dislocation of the cataract during or after the operation of couching, then much in vogue, necessitated its removal through a corneal incision. Petit, St. Yves and Daviel contributed each their share in bringing the operation of extraction into general use, but to Daviel in particular belongs the merit of devising and perfecting the operation of which he published an account in 1747. Brisseau's previous discoveries or rather demonstrations respecting the position and nature of cataract contributed no doubt largely to the adoption and progress of the new operation. In 1650 Reni Lasnier, a Paris surgeon, maintained that cataract resided in the crystalline lens, and in 1705 Brisseau demonstrated that such was absolutely the case. In the *Philosophical Transactions* for 1724–25, John Ranby the favourite surgeon of King George the Second records the dissection of two cataractous eyes which showed that the lens was the seat of the cataract. In the same volume is contained a Latin communication respecting the eyes of a soldier, who died in the Royal Hospital, Kilmainham, nine years after having been couched in both eyes by Caywood, a Dublin oculist; the eyes were examined by Professor, afterwards Sir Thomas

Molyneux, and no trace of the lens was to be found in either eye. In 1750 O'Halloran one of the most eminent surgeons of his day and practising in Limerick, published a treatise on the cataract prefaced by the significant sentence from Seneca, *veniet tempus quo, ista quæ nunc latent, in lucem dies extrahet;* in this philosophical essay the author shows the cataract to be in the lens. O'Halloran was amongst the first British surgeons to extract the cataract as is shown by his essay in the second volume of the *Transactions of the Royal Irish Academy* published in 1788; he advocated a corneo-sclerotic incision somewhat similar to that of von Graefe to which I have to refer, and suggested also the removal of the cataract in its capsule. Some of the earliest writings in the English language on the extraction of cataract are those afforded by Warner 1775, Chandler, Butter, &c. This operation has gradually been perfected and has now, I rejoice to say, superseded in all civilized countries the dangerous and reprehensible practice of couching or reclination—dangerous because even if temporarily successful the depressed lens having been forced through and broken up the vitreous humour and hyaloid membrane is placed lying in the bottom of the eye in contact with the retina, and pressing upon it, the choroid and ciliary body; in this new position it acts as a foreign body giving rise constantly to a slow insidious form of inflammation or even panophthalmitis, which destroys not only the sight of the operated eye, but also frequently that of the second eye through sympathetic extension of the irritation. Couching cannot be too strongly or too loudly condemned, and if mentioned at all in writing or teaching it should be alluded to solely as a matter of past history.

The operation of extraction has undergone various modifications and improvements; some authorities, such as O'Halloran, Butter, Earle, Benjamn Bell, as well as continental authorities recommending the sclerotic or the corneo-sclerotic junction as the best place for the incision, the majority of operators advising the cut to be made entirely in the transparent cornea; some made the section above, others below and again others at the side of the globe. Most operators made a large incision, some such as Wardrop, Travers, and Adams made a small section, Wardrop recommending an opening one quarter the periphery of the cornea. The operation for the removal of senile hard cataract as eventually practised, and which held undisputed sway amongst scientific men down to a few years ago, is known as the flap extraction, and is performed by making an opening in the transparent cornea to the extent of one-

half of its circumference, opening the capsule of the lens and expressing the cataract through the pupil and through the corneal opening. In cases of soft cataract in juvenile persons, Mr. Gibson of Manchester first broke up the lens with a needle, and after some time made a small straight or linear incision in the cornea through which he removed the broken up lens with a curette. Mr. Walker, also of Manchester, devised the grooved needle-knife here figured,

Fig. 1.

and which was originally described by Sir Wm. Wilde in this journal and by him introduced into practice. In recent traumatic cataracts the lens is quite soft and will readily flow out through the groove when the instrument is pushed through the cornea into the lens.

Flap extraction is amongst the most beautiful of operations, and yields when successful the most perfect results both as regard vision and appearances; cases occur in which it is difficult to recognize any traces of an operation; it requires however great steadiness and quiescence on the part of the patient, and it demands on the part of the operator considerable manual dexterity (only to be acquired by practice), keen vision, calmness and control of temper, and patience which the most untoward events will not ruffle or exhaust; a correct judgment and promptitude in action which will enable him to encounter successfully the various casualties which are likely to occur; he should possess moreover an extended knowledge of human character in general and of his patient in particular; and in addition to this list of personal qualifications he must be provided with an experienced assistant who should act with him in unison of mind and body, who should in fact be to him a third hand. By constant practice and long experience an operator may acquire such expertness that the operation becomes in his hands almost a certainty; but no dexterity and no experience can avert the sometimes disastrous consequences of the operation; the real dangers commence only after the completion of this operation, then ensue the long and weary days and nights of careful watching, and of alternating hopes and fears, a most anxious time for both patient and attendant, as I well know from my past experience. I will now very briefly refer to some of the chief difficulties and dangers during and after flap extractions, in even the most skilful hands.

There are certain measurements and indications laid down for the extent and position of the incision in the cornea, but it is found impracticable, except under chloroform and with fixation of the globe to make a perfect section in every case, it may result too small or too large, its position may be faulty or it may have to be completed or enlarged with the scissors or knife; the iris may be cut, vitreous may be lost, hemorrhage may occur; I have even seen the whole contents of the eye, iris, lens, vitreous, retina, and choroid projected through the wound. In order to express the cataract in the normal extraction the lens must be rotated to a great extent in its axis and pressed through the pupil; now by these acts considerable pressure must of necessity be exerted by the lens on the iris and vitreous humour, and in consequence of this bruising of the iris inflammatory changes are very apt to occur in that structure. In cataracts with soft periphery the soft matter is very likely to be scraped off and to remain behind the iris where it gives rise to iritic complication and neoplastic formations in the pupil. In some instances the vitreous is perfectly fluid, and the lens when freed from its capsule falls down into the bottom of the eye, from whence it is sometimes almost impossible to remove it. Another great difficulty in flap extraction is presented by adhesions of the iris to the lens. Of course unsteadiness on the patient's part and spasm of the ocular muscles will greatly complicate the difficulties of the operation. I have more than once seen the operation attempted and postponed to another day in hopes of the patient becoming more quiescent. The principal dangers which retard or prevent the successful recovery are non-union or reopening of the wound generally with and rarely without prolapsus of the iris, ulceration or sloughing of the edges of the wound, suppuration of the cornea, iritis, chorido-iritis, panophthalmitis; hemorrhage is of very rare occurrence after flap extraction, it does however sometimes occur, and if to any great extent results in loss of vision. It is but natural to suppose that a wound of such magnitude cannot be made with impunity in a non-vascular structure, like the cornea in persons advanced in life; owing to the great extent of the cut the cornea is peculiarly liable to destructive changes. It is a matter of great importance that the surface and edges of the wound should be in perfect apposition and heal by primary intention; practically it is found very difficult to keep the opposed lips of the wound in perfect co-aption; hence Dr. Williams, of Boston, has recently recommended the wound to be united by sutures; he states he has

obtained most satisfactory results from this practice. Suppuration of the eyeball commencing either in the cornea or in the uveal tract is of all the sequences the most fatal to the eye; and occurs even under the most favourable circumstances in about 7 per cent.; iritic complications are of very constant occurence and result in complete or partial closure of the pupil as also in neoplastic formations in it requiring secondary operations; non-union or reopening of the wound with prolapse of the iris retards the recovery very much and results in distorted pupil and large cicatrix of wound. Loss of vitreous during flap extraction results generally though not invariably in a broad and extensive corneal cicatrix and horse-shoe shaped pupil. Secondary hemorrhage, vomiting and diarrhea are amongst the rare sequences. Although flap extraction yields when successful as I have stated the most perfect results, results such as no other operation has attained, yet the dangerous sequences are so great and so many, that operators have been induced to seek for some more safe method of removing the cataract. About the year 1861 von Graefe, of Berlin, occasionally performed an iridectomy in cases where there had been any difficulty in expressing the lens or in which he had reason to anticipate an unfavourable recovery. A couple of years subsequently one of von Graefe's assistants, Dr. Schuft, now named Waldau, removed the lens by means of a spoon-like instrument through a comparatively small straight incision in the transparent cornea. It is right to mention, however, inasmuch as the spoon has become a generally adopted instrument that von Graefe claims having made use of such an instrument long before Waldau, and states correctly that after all the spoon is merely a modification of Daviel's instrument so long and still in use. Figs. 6 and 7 on plate 8 of Scultet's *l'Arcenal de Chirurgie*, 1675, might almost be taken as the prototype of the curette and spoon. The idea of removing the lens by an instrument introduced through the pupil to the back of the lens is not new, for Bell in his *System of Surgery*, 1786, recommends a hook for that purpose. Butter made an incision into the sclerotic with an instrument which opened forceps-like, seized and withdrew the cataract. To avoid bruising the iris and to avert subsequent inflammation as well as to facilitate the exit of the lens iridectomy afterwards became a feature of Waldau's scoop operation; the spoon underwent various modifications and improvements, Mr. Bowman's being I think the best, and eventually an operation known as the traction extraction of Bowman and Critchett came into general use. This operation

consists in making a straight incision into the transparent cornea immediately at its junction with the sclerotic, or better into the corneo-sclerotic junction itself, excising a segment of the iris, opening the capsule and then introducing the spoon between the lens and the vitreous and lifting out the cataract in it; the cut is made at the upper part of the globe by means of a triangular spear-shaped Jaeger's knife, bent on the flat and embraces nearly one-third of the corneal circumference. In consequence of the cut being made superiorly, the iridectomy, which is a necessary portion of the operation, is concealed by the upper lid—indeed as a rule I invariably perform iridectomy when undertaken for the relief or cure of glaucoma or in cataract operations at the upper portion of the iris, because the upper eyelid not only conceals the coloboma which might be considered unsightly, but also because it cuts off the peripheral rays of light which might prove dazzling and troublesome. Although some of my most successful cases eventuated from traction extraction I have discontinued the operation as it did not yield me the satisfactory results I had anticipated; I have had suppuration after it both in private and in hospital practice. I may mention incidentally that in one of these cases of suppuration which occurred in my practice at the Richmond Hospital I subsequently removed the cataract from the second eye by the old flap extraction with the most satisfactory termination. I regard the introduction of traction instruments behind the lens as a dangerous procedure more especially in elderly persons with weak fatty heart; such persons are indeed I think peculiary liable to suppurative inflammation after any extraction. Iridectomy has now become an almost indispensible feature of extraction no matter by what method; Mooren of Düsseldorf was in the habit of performing iridectomy some weeks prior to the flap extraction, but notwithstanding the very satisfactory results he obtained (only 14 losses out of 229 extractions), he has now abandoned his method for that of von Graefe. Removal of the lens in its capsule which I have referred to was practised, occasionally by Beer, Richter, and some of their pupils; Dr. Pagenstecher, of Wiesbaden, who has revived this procedure makes an iridectomy downwards. Jacobson executed a flap extraction with iridectomy following the expulsion of the lens—a proceeding I witnessed occasionally in von Graefe's clinique several years ago. Professor Macnamara, of Calcutta, after dilating the pupil with atropine and chloroforming the patient makes a straight incision in the temporal

side of the transparent cornea to the extent of half an inch, and while withdrawing the knife opens with its point the capsule of the lens; he then introduces a fenestrated spoon by which pressure is made on the edge of the lens, which causes the latter to tilt over in its axis and to lie in the scoop. I have not performed this operation, which appears to me very similar to Schuft's original scoop extraction, but I should fancy that the loss of aqueous humour and the irritation of the operation would tend in a large number of cases to contract the pupil, and that there must of necessity be a good deal of bruising during the manipulations; the professor states, however, that he obtains perfect results where the flap extraction utterly failed. I now come to the latest modification of extraction from which I have been led into this long digression by the great interest attaching to the history and literature of extraction in general.

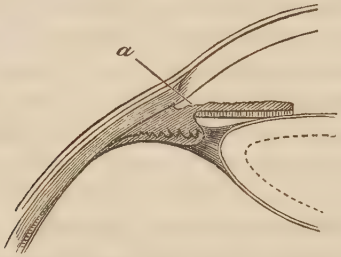
This modification, which is known as von Graefe's extraction by peripheral linear section, or modified linear extraction has been adopted by many operators, and as it appears to answer the requisite purposes, and seems to me to afford a safer means of removing cataract than the flap extraction, I would wish to bring it under the notice of the members of the Irish profession, who may have few opportunities in country districts, and little leisure for consulting special works and periodicals. It is due to Dr. Taylor, of Nottingham, to mention here that quite independently of von Graefe he adopted in isolated cases a section almost identical with that I am about to describe. Dr. Taylor performed prior to the section what seems to me a very small and incomplete iridectomy, as he made it through such a small incision in the cornea as was "just large enough to admit a small blunt hook." His notice is contained in the April number for 1866, of the *Ophthalmic Review*, a publication which has I regret to say ceased to exist.

Von Graefe endeavours to avoid the principal dangers of flap extraction, by making the section as small as possible compatible with the exit of the lens, in such a structure as is least liable to suppurative inflammation, and in such a position and manner as will allow of the most facile delivery of the lens and of the most perfect coaption of the surfaces of the wound, as well as by adopting such precautionary measures as are likely to guard against subsequent destructive inflammation. These objects he attains, I conceive, by executing a small nearly straight section in the sclerotic border, and corneosclerotic junction, nearly opposite the

margin of the lens, by making a conjunctival flap which shall cover the wound, and by performing an iridectomy.

The accompanying cut copied from von Graefe's essay in the

Fig. 2.



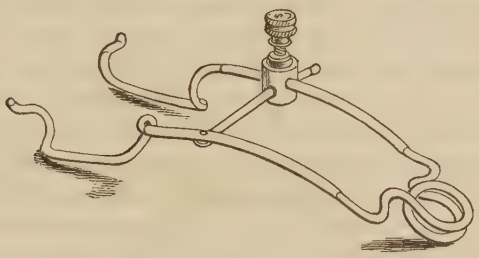
Archiv für Ophthalmologie for 1865, illustrates the relative position of the parts, the line *a* representing the incision. It is well known that wounds in the sclerotic and corneo-sclerotic junction are less liable to suppurative inflammation, and that they heal more readily than those in the transparent cornea; the external wound measures $4\frac{1}{2}'''$—$5'''$[a] and the internal one $4\frac{1}{2}'''$, the height of the flap does not exceed $\frac{1}{4}'''$ whereas it measured $2'''$—$2\frac{1}{2}'''$ in the old flap extraction. The wound in von Graefe's operation, though strictly not a linear one, may for all practical purposes be regarded as such; its surfaces lie in exact apposition, it is covered over by the conjunctiva, and is in every respect in the most favourable position for immediate union. By a reference to the illustration it will be seen that the opening is as nearly opposite the margin of the lens as is possible without interfering with the ciliary body and processes. The manifest advantage of this position as regards the delivery of the lens, is that when the iris is removed, the capsule opened and pressure made at the opposite side of the globe, the lens glides easily and naturally into and through the wound; the disadvantage is that the internal corneal margin being cut across the zonula is thereby deprived of its support, and there arises a tendency towards escape of vitreous, a circumstance which has occurred in about twenty-five per cent. of the cases I have operated on.

The operation is performed as follows:—The patient is placed in the recumbent position, on a couch or bed opposite a good light, the eye not to be operated on is covered or bandaged

[a] The foot is expressed by (′), the inch by (″), and the line by (‴).

down, the eyelids separated by the spring speculum here figured and the globe drawn downwards by a toothed forceps applied

Fig. 3.

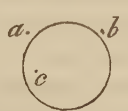


immediately below the inferior margin of the cornea; in fixing the eyeball a broad fold including the sub-conjunctival cellular tissue and even the insertion of the inferior rectus if feasible should be grasped between the blades of the forceps, for the conjunctiva alone affords often but a slight hold and is liable to be torn through. In fixing the globe it should be rotated directly downwards, otherwise the section and consequently the iridectomy will fall towards one side and the resulting coloboma will be exposed to view and appear unsightly as well as give rise possibly to disturbing dazzling. When the operator has thus steadied the eyeball he holds the knife here figured in his other hand, its flat surface parallel to the plane of the iris, its cutting edge directed upwards

Fig. 4.

and inwards, and inserts its point into the sclerotic $\frac{1}{2}'''$ from the corneal margin at *b*, Fig. 5, copied from v. Graefe, which represents

Fig. 5.



the left eye, and pushes the instrument obliquely downwards and inwards into the periphery of the anterior chamber towards *c*, in order to secure a large internal wound. When the knife has entered the chamber in this direction, to the extent of $3'''$ or $3\frac{1}{2}'''$ its handle is depressed and the point thus elevated is directed

towards the counter puncture *a*, and pushed onwards beneath the opaque cornea into and through the sclerotic border. When the point of the knife has penetrated the sclerotic and appears either underneath or outside the conjunctiva, the cutting edge of the instrument should be turned very slightly forwards, and the section completed in the corneo-sclerotic junction, either by a sawing motion or an onward thrust. The blade of the knife then lies free immediately underneath the conjunctiva, and should be still further rotated with its cutting edge forwards or even forwards and downwards and the conjunctiva divided in this direction. By this means a conjunctival flap of not too large dimensions is formed which subsequently covers the wound. In making this section the knife should be entered at the extreme periphery of the anterior chamber, and should leave it in a similar position; care must however be taken not to introduce the knife too far downwards and inwards into the chamber before reaching the counter-punctuation and not to direct the point of the knife too far backwards, otherwise a large gaping wound will be made in the sclerotic implicating the ciliary body and exposing the vitreous humour, neither should the counter-puncture be in the transparent cornea; if a faulty puncture or counter-puncture be made the point of the knife should be withdrawn and inserted in the desired position. The outer wound is almost altogether in the true sclerotic tissue while the inner is in the corneo-sclerotic junction. It happens sometimes that the point of the knife after dividing the sclerotic pushes the conjunctiva before it instead of piercing it and the aqueous humour escapes into the bag of the conjunctiva; this circumstance should not alarm us, nor prevent the section being quietly completed, but when the knife has been cut out the conjunctival opening should be enlarged with a scissors down to the point of counter-punctuation. With the completion of the section terminates the first step of the operation; the forceps is then given to the assistant who draws down the globe and the operator turns the conjunctival flap back over the cornea. The prolapsing iris being thus fully exposed is seized in the iridectomy forceps near one angle of the wound drawn but very slightly forwards and cut through with a straight or angular scissors as close as possible to the corner of the wound; its ciliary attachment is then freed by a touch of the scissors or torn through by drawing the forceps with the iris in it to the opposite angle, where it should be divided also as close as possible to the corner of the wound. In quiescent patients with moderately

prominent eyes I sometimes make the iridectomy without having the globe fixed. It is very important to remove the iris up to its ciliary attachment as completely as possible, especially at the angles, for otherwise it is very likely to prolapse, retard the healing of the wound, and give rise subsequently to a cystoid cicatrix, which is peculiarly liable to become irritable. It requires some little care also to avoid cutting the conjunctiva with the scissors, it should therefore be carefully turned down over the cornea before the iridectomy. On completion of this second step the globe is released by the assistant, and if there be no complication the third step that of opening the capsule is to be forthwith proceeded with. If there be hemorrhage into the anterior chamber from the iris or conjunctiva it should be removed before lacerating the capsule, gentle upward movements of a spoon or curette over the front of the cornea and slightly opening the wound will generally suffice to remove the blood. The laceration of the capsule seems to me the most difficult part of the whole operation and requires considerable dexterity and judgment. The operator fixes the eye, introduces the cystotome on the flat as far as the inferior border of the pupil where he gives the instrument a half turn, whereby its cutting sickle-shaped edge comes into contact with the convexity of the lens, and in this position he draws the instrument upwards and thus opens the capsule; it is well to make two cuts starting from one and the same point inferiorly—the one directed upwards and inwards, the other upwards and outwards; an incision along the periphery corresponding to the section is also advisable. In this manipulation great care is to be taken not to displace the lens—a thing difficult to avoid in old cataracts with tough degenerated capsules. If the lens be accidentally disturbed it should be replaced *in situ* by a hook or curette, otherwise its faulty presentation to the inner wound will cause difficulty in its delivery. If there be adhesions of the iris to the lens they should be divided with the cystotome. The fourth and last step consists in the expulsion of the cataract; the fixation forceps is shifted to one side of its former position, the convex smooth surface of a caoutchouc or tortoise-shell spoon which will not injure the corneal epithelium is placed on the lowest part of the cornea and pressed against it in a backward and upward direction; this pressure causes the lens to start and should be continued steadily backwards until the lens is fairly in the wound, when the pressure should be directed upwards by gliding the spoon along the cornea, following the movements of

the lens until the whole cataract has been expelled; should portions of the soft cortex be stripped off and remain in the pupil they are to be pressed out by the action of the spoon on the cornea and by opening the wound. If there be any difficulty in expressing the lens the posterior lip of the wound may be slightly depressed while the pressure is continued below—an assistant fixing the eye; it is advisable at this stage of the operation however if fixation be necessary for the operator to control it himself, as improper pressure might easily cause loss of vitreous. In the majority of cases of ordinary cataracts this pressure properly directed will suffice to express the lens; in some instances however notably where there has been much loss of vitreous or where the vitreous is fluid it fails, and we must then have recourse to traction instruments, such as hooks or spoons; v. Graefe's hook bent in the stem so as to adapt itself to the convex posterior surface of the lens is introduced on the flat, given a half turn and inserted into or beneath the lens and the latter withdrawn, or a spoon is made use of as in the traction extraction. Where the vitreous is known to be fluid we should remove the lens with the hook without having recourse to pressure. Before bandaging the eye we should satisfy ourselves as fully as possible by testing the vision and otherwise that all portions of the lens have been removed: after taking the speculum away and allowing some time to elapse we should gently rub the upper lid over the globe by which means often small particles which had previously remained concealed behind the iris will come into view; these as well as pieces of capsule should be carefully removed as they are, if left behind, most likely to cause iritic complications and neoplastic formations; the time thus spent no matter how long should not be grudged or considered illspent, for on the *complete* removal of the lens depends in a great measure the ultimate success. I am in the habit of testing the vision at this stage; in the ordinary cases where a cataract has been perfectly removed the patient should recognize and distinguish faces. All filaments of blood and particles of lens are to be removed from the wound by means of a curette. In one instance of normal operation in which vision was as perfect as possible under the circumstances a small clot of blood remained at the inner angle of the wound, and with a view of removing it I seized it with the iridectomy forceps as I had often done previously; at the very moment however the patient made a sudden movement with her head and I saw the iris stretching and, before I had time to open the forceps, the ciliary attachment at its

lower and inner side gave way and blood poured forth which quickly filled the anterior chamber; the reason of this was the fact that I had unawares seized in the forceps a piece of prolapsing iris which had been concealed by the clot of blood. I may mention that notwithstanding this serious and avoidable accident the patient after some months recovered very useful sight. The error I committed is instructive in several particulars—but more especially in showing how careful we should be to remove the iris completely at the angles of the wound; it shows also perhaps the necessity for having the patient's head held and steadied by an assistant during delicate operations on the eye; as a rule I do not adopt this practice in extraction preferring to leave it to the patient's own sense not to stir, neither do I as a rule allow the patient to hold the hands of bystanders as I think it conduces generally to spasmodic movements which would otherwise not take place; the trust and confidence reposed in the patient are usually responded to by quiescence. When the operation has been completed a piece of fine linen or muslin about $5'' \times 3''$ divided up the centre to the extent of about $2''$ is placed over the eyes the undivided portion on the forehead the division being occupied by the nose, the hollows around the eye especially at the inner angle are then carefully filled up over the linen by cotton wool and the cotton packed on so that its surface shall project beyond the level of the orbital margin and nose; over this is placed Liebreich's bandage and tied pretty tightly. By this means a uniform and equable pressure is kept up on and around the eyeball, and the bandage is so constructed that no movements of the patient's head can displace it, while it admits of being opened and reapplied and of the dressings being changed without disturbing the patient.

This modified linear extraction is not difficult of execution in full ordinarily prominent eyes such as are met with in Germany and in England; in Ireland however the eye is, particularly amongst the native Celtic population, generally small and very deep-set with prominent overhanging eyebrows, and these circumstances render the operation often extremely difficult; if a line be drawn from the eyebrow to the lower orbital margin and another from this line to the cornea the latter one measures often $6'''$, in one instance of flap extraction recorded in my notes it measured as much as one English inch, in this case the space from angle to angle measured not quite $\frac{3}{4}''$ and the natural opening of eyelids $\frac{1}{4}''$; these measurements which though not common are yet

not exceptional, illustrate one of the difficulties we have to contend with in eye operations in this country, and explain the smallness of Sir Wm. Wilde's flap extraction knives, the cutting edge of which is often only $\frac{3}{4}$ of an English inch or 9‴ long, the whole blade being 1 inch, while the shortest of the knives purchased by me in Germany measures 16‴ in the blade, and 13‴ in the cutting edge, the longest 18‴ in the blade and 15‴ in the cutting edge; such instruments as these latter would I need scarcely remark be quite unsuited to the generality of eyes in this country. In cases such as I have alluded to no matter how far we may rotate the eye downwards the very prominent eyebrow interferes with the execution of v. Graefe's section, and I have been endeavouring to obtain some curved or angular modification of v. Graefe's knife to meet the exigencies of the cases. Another difficulty is presented in the speculum; the best form for all purposes is that already figured which is the one for the left eye, that used for the right side having the regulating nut on the opposite arm; this nut should in v. Graefe's operation be always next the lower lid so as to be out of the way of the knife; a good practical speculum for this new extraction is still a desideratum. I have employed the rack screw speculum of Mr. Noyes, of New York, but have not found it satisfactory as it is apt to slip out, perhaps at the most critical part of the operation. The blade of v. Graefe's knife represented in Fig. 4, the natural size should be as narrow as possible compatible with solidity and strength, its point and edge should be very sharp; in sharpening instruments cutlers are apt to lessen their size and strength, and this knife is very likely to suffer in the process; in one case I had some difficulty in making the section from the fact of my knife being thus thinned and weakened. In the ordinary knives the handle where it joins the blade is usually cut sharply off nearly at right angles, after lowering the handle and pushing the instrument onwards towards its counter-puncture it has been more than once completely arrested in my hand by being caught at this part of the handle by the crossbar of the speculum; I therefore have had this abrupt termination altered and the handle tapered off as shown in the woodcut. It is advisable also to have some mark on the handle by which to distinguish readily the cutting edge from the back of the blade; it has happened that the instrument has been introduced into the eye with its cutting edge directed downwards and outwards instead of upwards and inwards. The speculum requires some

little attention from the assistant, thus it should not be allowed to press upon the globe, and its crossbar should be watched so as not to interfere with the play of the knife.

I must leave the question concerning the advisability of chloroform administration to the individual operator; if employed complete anesthesia should be produced, otherwise the unconscious struggling and muscular spasms during incomplete anesthesia will be worse than the unsteadiness of the waking and conscious individual. Chloroform renders the majority of operations much easier of execution and ensures probably in delicate eye operations greater accuracy and possibly therefore more perfect results. From my early training in St. Mark's Hospital and in Vienna I have acquired the habit of dispensing with the use of chloroform in non-painless and indeed in most eye operations and have not given it in any single case of v. Graefe's extraction. Although I have not as yet employed chloroform in this extraction I do not condemn its occasional use nor would I hesitate to use it were I pressed to do so and there were no contra-indicating circumstances; beginners and unsteady hands will find chloroform of the greatest assistance.

I do not adopt any preparatory treatment, and prefer operating as soon as possible after informing the patient of the advisability of an operation; in hospital it is desirable to operate at once for many reasons, not the least cogent of which is the fact of the patient being sometimes terrified by the alarming and often absurdly false statements made to him respecting operations by patients already in the hospital. I have not unfrequently traced the nervousness and unsteadiness during an operation to such sources. When practicable I operate on the day of admission or the day after, and as a rule only on one eye at a time. I have operated on the second in a week after the first, but two to three weeks at least should I think intervene between the two operations. I perform all cataract operations with the patient in the recumbent position I myself standing behind and above that head, being ambidexter I use the knife, needle, or forceps with the right or left hand indifferently according to the side to be operated on. I prefer the standing position as I thereby obtain greater command of view and of action than in the sitting posture; there are also some advantages I think in being ambidexter and I would advise all beginners to practice eye operations on the subject with both hands. During this as well as flap extraction it is well

to divert the patient's attention from the operation by engaging him in conversation on some interesting or to him familiar topic.

In the after treatment we must be guided by circumstances. I always have the patient placed in bed immediately after the operation; the first night I almost always give a full opiate, and throughout the treatment I order a generous dietary, with wine, brandy, or porter; the patient should not be restricted to any one position, but may be allowed to lie in the most convenient or usual posture; he may be allowed to sit up in bed on the second day and out of bed on the third or fourth day if the hospital arrangements are suitable. The bandage should be opened and the linen changed at least once a day, if there be much discharge from the eye it may be necessary to change it more frequently—but it is desirable to have the eye perfectly quiescent for the first twenty-four hours after the operation; I am in the habit of instilling atropia solution on the second day after the operation, and if any iritic complications exist earlier and several times a day. The compress bandage should be kept on for at least three days, the pressure being somewhat relaxed on the second day after the operation, unless in cases of sanguineous effusion or threatened suppurative inflammation. The bandage should be dispensed with gradually and it is well to guard against any possible accidents by replacing it at night during the whole of the first week. In the day-time the light in the apartment is to be moderated and the patient should wear coloured glasses for a fortnight at least after the operation, too early exposure of the eye is often punished by inflammatory symptoms. I have in very rare instances had recourse to depletion by leeching or cupping,—when inflammatory processes did arise I contented myself for the most part with the frequent instillation of atropia, the continuance or the relaxation of pressure, the application to the brow of an ointment composed of 1 drachm of extract of belladonna and 3 drachms of mercurial ointment and the administration of suitable internal remedies such as purgatives, tonics, stimulants, alteratives and absorbents, and hitherto my results have been quite satisfactory. I have had a few instances of hemorrhage occurring some days after the operation—in one a couple of hours after leaving off the bandage; when blood is effused into the anterior chamber the compress bandage should be retained firmly applied until the blood has been absorbed. Slight serous chemosis is of not uncommon occurrence but so far as my observations extend is not by any means a

dangerous symptom; it has occurred generally without any swelling or discoloration of the lids and appears often due to the sub-conjunctival section and the escape of the newly-formed aqueous humour beneath the conjunctiva; in most of those cases the chemosis was confined to the lower part of the globe; the treatment consists in aggravated cases in snipping one or more pieces out of the raised conjunctiva with a curved scissors and allowing exit to the fluid.

Fig. 6.

The accompanying cut shows the condition of the eyes after the operation in a man aged 69. In order to illustrate the peculiarities of the operation fully I have drawn the right eye in its normal position, the left with the lid raised so as to expose the whole iridectomy as well as the track of the wound, indicated by the black line above the cornea; at the time the drawing was taken the right eye had been operated on a month, the left a fortnight; a small portion of the capsule was opaque in the left pupil and there was a good deal of vasularity about the eyeball; the iridectomy, as may be perceived, is rather large, but this is a fault on the right side; the patient presented somewhat sunken eyes and the shaggy eyebrows hung down over the eyes concealing the eyelids; this latter condition together with the frown peculiar to cataractous persons which was so marked in this individual is well rendered on the right side in the engraving which has been admirably executed by Mr. Hanlon.

The average length of time spent by the patients in the hospital from the date of operation until dismissal was 21 days, the shortest period having been 6 days, the longest 62 days; the long periods refer to patients who had both their eyes operated on. These figures contrast most favourably with those of flap extraction in the same hospital, the average number of days for which was 43 and has reference only to one eye—the second having as a rule never been operated on during the same visit to the hospital.

The apparent immunity from the dangerous consequences of flap extraction has led me to execute the peripheral linear extraction even in the most hopeless cases which formerly I would not have interfered with, and hitherto with the most satisfactory results quoad the mere operation and the recovery of the eye from its effects. Wherever there exists the faintest chance of improvement or where urged by the patient, who may have come a very long distance in expectation of an operation, I never hesitate now to give him the benefit of the doubt, and up to the present I have no cause to regret having done so in hospital practice, on the contrary the operation has in a few instances benefited the individual contrary to expectation. I have also used this operation with advantage in cases affected with granular conjunctivitis, a disease which is so prevalent in this country that it is practically impossible under present arrangements to exclude it altogether from the hospital. I entertain very decided views as to its contagious nature and consider that patients affected with it should be rigorously excluded from contact with individuals suffering under other ocular diseases.

This new extraction is also applicable to immature cataract and to cataracts with adherent pupil; one of its great advantages is its applicability to cataracts in middle aged persons or even youthful individuals; I have operated by this method on a few below 30 years of age and in one instance on a boy 11 years old affected with traumatic cholesterine cataract; the youngest age at which flap extraction is recorded in my notes as having been performed at St. Mark's Hospital is 38. The linear peripheral section may also be employed in other operations with advantage.

The crowning feature of this new method of extracting consists in the very rare occurrence of suppuration; as yet there has not been a single loss from this cause in the hospital; the only case of suppuration I have had occurred in my private practice in a gentleman whose other eye had been unsuccessfully operated on elsewhere some years previously.[a] Contrasting v. Graefe's new modified linear extraction with the old flap extraction it has I conceive the following advantages: it makes less demands on the volition and quiescence of the patient and requires neither the great experience nor the manual dexterity on the part of the operator nor the trained assistance requisite for flap extraction; there are fewer and

[a] Since writing the foregoing I have had to deplore the first and only loss by suppuration which has occurred in sixty cases of the operation at St. Mark's Hospital.

less grave casualties during the operation, and there is no difficulty with the iris during or after the operation; the size and position of the wound are matters of almost certainty; the lens can be always removed no matter how fluid the vitreous may be; the wound heals readily, there is great immunity from suppuration, the time occupied in the recovery is diminished by one-half, the after treatment is exceedingly simple, the operation is applicable to a greater number and larger variety of cases than the flap extraction. I may mention that several friends and pupils to whom I have shown and explained this operation and who then tried it themselves speak in the highest terms of the satisfactory results they have obtained by its means. As is well known it is necessary to substitute a glass lens externally for the cataractous lens we have removed; the focus of this cataract glass for distant objects is after flap extraction as a rule about $3\frac{1}{2}''$, and for reading from $1''$ to $2\frac{1}{2}''$; in this new linear extraction a much lower powered glass answers, thus $4\frac{1}{2}''$ and $5''$ are very common walking glasses and some of my patients get on very well without them altogether; whether this be any advantage I cannot at present say, it shows however that this new operation does not alter the corneal curvature to the same extent as is done in the flap extraction.

In conclusion I believe that in this new operation we have a much safer and more speedy means of curing cataract than in any method hitherto practised, and one which is particularly applicable to hospital practice. I do not however advocate its adoption to the entire exclusion of flap extraction which may still be retained with or without iridectomy in some cases especially in private practice.

ART. XIV.—*Transfusion Successful in a Case of Post Partum Hemorrhage.* By THOMAS E. BEATTY, M.D., M.R.I.A.; M.D., T.C.D., Hon. Causa; Ex-President of the King and Queen's College of Physicians in Ireland; Ex-Professor of Midwifery, and formerly Professor of Medical Jurisprudence, Royal College of Surgeons in Ireland.[a]

MRS. ———, aged twenty-four years, a very handsome, tall, well-made lady, had a premature confinement of her first child at the fifth month, in January, 1867. She became again pregnant, and was delivered of a large healthy boy, at the full time, in July,

[a] Read at Obstetrical Society April 9th, 1870.

1868. She nursed her child for nine months, and was again pregnant in September, 1869. Towards the end of November of that year, she had discharge of blood from the vagina, without pain, which came on every night, and lasted till morning, in small amount, and then disappeared during the day. She was kept in bed, and directed to apply vinegar and water on her napkins, and to take five grains of gallic acid two or three times during the night, as required. Sometimes two or three nights would pass over without any bleeding, and then for a few nights it would return. In January of the present year the intervals became longer, but still there were occasional returns, which were always checked by the gallic acid and cold applications. In this way the month was passed. She became larger, and the uterus could be felt above the pubes. A distinct placental souffle could be heard about the middle of the mouth, but the fetal heart was not audible. She was now allowed to get on a sofa in her room during the day, and there was no further hemorrhage. On the 18th of February, finding herself so well, she went to her drawing-room, where she still observed the recumbent posture, and did the same for four days.

On the morning of the 22nd of February I was sent for at 7 o'clock, and on my arrival I found that she had been seized with labour an hour before, accompanied with profuse hemorrhage. She had no nurse; her mother was alone with her. A six month child was expelled, alive, and lying in an ocean of blood. The patient was pale, pulseless, and cold as marble. The placenta was still in the uterus. Upon making pressure on the abdomen, a very large coagulum of blood was expelled from the vagina; the uterus felt firm; and on tracing the cord up, the os uteri was found closed to the size of a two-shilling piece, holding the placenta within. A slight draining of blood continued to flow. The only stimulant at hand was a bottle of sherry, of which I gave her three glasses in rapid succession. It was very plain that she was in imminent peril, and I despatched a messenger to summon Dr. Denham, who came to my assistance without delay. Before his arrival, I gave her a drachm of ergot in more wine, and had jars of hot water applied to the feet. On consultation with Dr. Denham, we agreed, that as there was still a draining of blood going on, it was necessary to remove the placenta. This I requested him to do, as his hand was smaller than mine, and would more easily enter the uterus, which was only developed to the sixth month of pregnancy, and the os uteri was closed upon the umbilical cord. With very

considerable difficulty this was accomplished, while I took charge of the uterus above, and made pressure to insure its contraction. A quart of vinegar and water was thrown up the vagina with a syringe, and all bleeding then ceased. Our endeavours were now directed to restore the exhausted vital energy of our patient. A pint of warm beef-tea and a glass of brandy were thrown up the rectum, and hot brandy and water were given by the mouth freely. Strong beef-tea was swallowed at intervals by our patient, but nothing seemed to revive her failing powers. The pulse never returned, and the same corpse-like coldness of the surface continued. Quart bottles filled with hot water, and covered with flannel, were placed in the axillæ, and along both sides, and under the knees, while the hot jars were kept to the feet. More brandy and beef-tea were thrown up the rectum, and were taken freely by the mouth, the stomach retaining everything. Sighing and tossing about began, and signs of sinking became manifest. It was now 1 o'clock in the day. Six hours had elapsed since her delivery, and no appearance of amendment had yet manifested itself. I felt convinced that she must die unless her downward course was checked, and I saw no other chance for her but transfusion. I knew Dr. Robert M'Donnell (who fortunately lived close at hand) had the apparatus for transfusion, and was skilled in the operation, and I summoned him and Mr. W. Colles, and Dr. Denham to a consultation. We met at 2 o'clock, and I laid my views before them. They all saw the urgency of the case, and the necessity for the operation. But just then a slight flickering of pulse was felt at the wrist, and the carotid artery gave a little stronger impulse to the finger, and it was judged right to wait some time longer. The pouring in of nourishment and stimulants was continued, and the external warmth was continued. Up to 6 o'clock matters remained in the same state, but after that every bad symptom became manifest. She became insensible; the gasping and sighing were continual, and the restlessness and tossing were constant; the countenance became ghastly, and the jaw dropped. All trace of pulse had long vanished, and death seemed very near at hand. Finding matters in this state, I determined not to let her die without making the only effort that, in my mind, could save her, and I again summoned the same gentlemen to her aid. Mr. Colles and Dr. R. M'Donnell arrived at 9 o'clock, but Dr. Denham was not able to attend.

It was now too manifest that she had not many minutes to live,

and we proceeded to perform transfusion. Her husband readily offered to furnish the blood, and about ten ounces were drawn from his arm by Dr. R. M'Donnell. This, as it flowed into a common bowl, was agitated briskly with a glass rod, in order to separate the fibrine; and the agitation was kept up after the full quantity had been drawn, and for some minutes after, until a large coagulum of fibrine adhered to the glass rod. During all this time the bowl was kept floating in a basin of hot water. The blood was then strained through a muslin cloth, and all the fibrine was thus separated. All this was done in the parlour, and the blood thus prepared was carried up to the patient's bedroom. The difficulty of the operation now commenced. The patient was as a corpse, bloodless; there was no trace of a vein to be found in her arm. A ligature tied round above the elbow showed nothing. By feeling cautiously, what we imagined to be a vein was perceived. Dr M'Donnell, who performed this operation with that skill, coolness, and dexterity for which he is remarkable, now adopted the expedient of pinching up a fold of skin at the bend of the arm, and then running a narrow bistouri through it, split it up, leaving a wide gaping wound in the skin, which displayed the veins at the bottom, resembling small flat dead earth-worms. The median basilic appeared to be the largest, and under this he passed a long acupressure needle, so as to elevate the vein, and prevent it escaping during the remaining steps of the operation. The difficulty now was to open this small, flat, empty vein without transfixing it. This he most dexterously accomplished by means of a fine pointed pair of forceps, with which he seized the anterior coat of the vein, and then with a fine tenotomy knife, the back of which was kept in contact with the vein, he succeeded in inserting its point into the vein, and slit it up above the point at which it was crossed by the subjacent needle.

The bowl of blood (which, during this necessarily tedious proceeding, had been kept floating in hot water) was now brought to the bed-side. The injecting apparatus was of the simplest construction. A common brass enema syringe that holds about two ounces such as is used for self-administration, was fitted with a fine india-rubber tube coming off from the side. This was about a foot long, and terminated in a fine glass tube four inches long running to a fine point at its extremity.[a] I held the bowl containing the blood

[a] The glass tube was used in order to facilitate the detection of any bubble of air, if such should gain entrance into the apparatus.

with my left hand, and with the right I held the brass syringe standing upright in the blood, and keeping the lower end of it well pressed to the bottom so as to prevent the possibility of any air gaining access to the interior. Mr. Colles worked the piston slowly and cautiously. The instrument was filled with blood till it ran out of the nozzle, and Dr. M'Donnell proceeded to introduce the glass tube into the vein; but this was very difficult to effect, for although a very sufficient opening had been made into the vein, it was so flat and collapsed that some time elapsed before he could accomplish his object. At length he succeeded, and Mr. Colles urged the blood forward by lowering the piston. About six or seven ounces of blood were thus poured into the system of the patient. It was intensely interesting to watch the effect produced by the introduction of this vital fluid. The first change I noticed was the improvement in respiration, the long laboured, gasping, sighing effort that had been so distressing to witness became more calm and like what natural respiration should be. This began to appear when about half the quantity of blood had been injected. The countenance became less ghastly, and imminent death seemed warded off when the last portion of blood had entered her vein. The restlessness and tossing still continuing, I gave her thirty drops of Batley's solution, and repeated the dose in half an hour; a third dose had the effect of producing sound sleep, from which in six hours she awoke warm and conscious, with a distinct though very fast pulse. She asked for food, and ate a good breakfast of tea and toast. 23rd, 12 o'clock.—Has had beef-tea and brandy twice; says she is quite well. Her countenance was bright and cheerful; pulse 140, small. She now complained of soreness of both hips, and on examining them, both were found to be burned by the hot bottles so freely used on the previous day. Two of these had escaped from their woollen covers, and during the tossing and insensible period had come into contact with the skin covering the hips. The right hip was most extensively burned, the slough which was subsequently detached from it left a deep ulcer, eight and a half inches in length by five and a quarter in breadth.[a] The burn on the left hip was about quarter the size. This will give a good idea of the condition of insensibility in which this patient was during the latter part of the previous day and night, up to the time of the operation. The low state of vitality to which she was reduced, contributed no doubt

[a] As I write now on the 8th of April, a large ulcer still exists on the right hip.

to intensify the effect of the hot bottles, for it is hard to imagine that water even at the temperature of 212° could produce so very deep an eschar as was the result of this burn.

10 o'clock, p.m.—She has slept all day from time to time, and has taken plenty of nourishment. I left a draught of muriate of morphia to be given if necessary.

24th, 8 o'clock, a.m.—She slept all night without any narcotic; pulse 120; looks bright, and makes no complaint, but of the hips; the arm gives no annoyance. Six p.m.—Has had beef-tea and toast freely all day; the bowels acted well, and slight lochia are present.

25th, 8 o'clock, a.m.—Had a very good night; pulse 115; ate a hearty breakfast; bowels acted well; voice strong; countenance good; no complaint of the arm, which healed up most kindly in a few days.

26th.—Another excellent night; pulse 100; ate half a roast chicken for dinner, and drank a glass of sherry.

From this time she progressed steadily, and after being out several days to drive in a carriage, she left town for the country on the 6th of April.

A most important point in the history of this case is the simplicity of the apparatus with which it was performed. A few shillings would cover the cost of the entire. Like many other highly important operations it is all in the using, not the costliness of the tools. It serves to show that there is no such difficulty in safely performing transfusion as we have been led to imagine, but that when the principles which should govern the operator are fully comprehended, it rests upon his dexterity and skill to carry them out, and not upon a complicated and unwieldy piece of machinery. Dr. M'Donnell accomplished, with his simple apparatus, what others possessed of elaborate machines have failed in achieving. This example of what may be done by good brains and skilful hands will, I hope, induce others to undertake the operation of transfusion, but not without a full knowledge of the essential points to be looked to, and avoided. In reading the history of the operation just detailed these points will all appear, and I will not prolong the communication by their repetition.

ART. XV.—*On the Hydrate of Chloral as a Hypnotic and Nervous Sedative, especially in Gynecological Practice.* By THOMAS MORE MADDEN, M.R.I.A.; Licentiate of the King and Queen's College of Physicians, Ireland; Member of the Royal College of Surgeons of England; Assistant Physician to the Rotunda or Dublin Lying-in Hospital; Corresponding Member of the Gynecological Society of Boston, &c., &c.

HAVING had an opportunity of testing the effects of hydrate of chloral, the new hypnotic and sedative, in a great number of cases, I venture to hope that the following observations, being a *résumé* of my notes of its action in several cases, may be of interest to those who have not employed this new remedy on an extensive scale.

Before entering on the consideration of the therapeutic value of chloral, as exemplified by the cases in which, with the approval of Dr. Johnston, Master of the Rotunda Hospital, I have administered this remedy in the gynecological wards of the lying-in hospital, as well as in private practice, I may premise that having no predisposition in favour of or against the new drug, I have given it a fair trial, and now submit the notes taken as to its effects in some of the cases in which it was administered, whether they be in favour of or against the supposed value of this addition to our therapeutic resources.

In many instances, those who have taken an active part in introducing new remedies into practice appear to have fallen into the not uncommon error of taking the *post hoc* for the *propter hoc;* and the experience of any one conversant with periodical medical literature, for even a few years past, will find highly educated and accomplished physicians extolling the virtues of novel medicines, which, within a very short time, have fallen into well-merited dissuetude. The confidence of the physician who administers a new drug of supposed extraordinary powers in the novel remedy is oftentimes reflected by the patient, who, being perhaps minutely questioned as to the effects which it was hoped might be produced by the medicine, begins to imagine what he wishes may be truly effected by the curing pill or potion; and, such is the wondrous power of the imagination over bodily sensation, that he oftentimes feels for the moment all the benefits which have been ascribed to the medicine, and unconsciously deceives his physician as well as

himself Anxious to avoid this error, I have endeavoured to test the new remedy, so as to exclude, if possible, this source of fallacy. I have, therefore, in a certain number of cases, first given the narcotics and sedatives usually employed, and have then compared their effects with those of hydrate of chloral, without letting either the patient or nurse know of the change of medicine, and I have then given some inert substance in draughts similar in form and taste to those previously prescribed, and had them administered with all the formalities with which the patient had been familiarized when taking the chloral for the purpose of the experiment; and I have thus been able to say how far the phenomena that followed were really produced by the medicine administered or not.

The great number of alleged new remedies which now-a-days are almost daily added to our list of the Materia Medica, already as long, and, in some respects, nearly as useless as the catalogue of the ships in the Iliad, should act as a preventive to our resorting to medicines simply because they are novel, unless they expressly accomplish new purposes or fulfil more advantageously those already attained; for, the unnecessary multiplication of medicines and preparations, far from adding to the resources of the practitioner, becomes, as Sir Henry Holland justly remarks, "an incumbrance to the practitioner and an impediment to the progress of the science."

Chloral, however, does promise to accomplish new purposes in the hands of the physician, as well as those of the surgeon, and to fulfil more advantageously others already attained.

The effects claimed for chloral by Liebreich, Richardson, and others, are:—1. That it produces sleep. 2. That it brings down the animal temperature. 3. That it removes sensibility. 4. Causes muscular relaxation. 5. Acts with extreme rapidity. 6. Does not require an increased dose to produce its effects, even after long use. 7. Has no counter-indication to its employment. 8. Is not followed by any unpleasant after-effects.

According to Liebreich, the physiological action of chloral is entirely due to the decomposition of the substance in the system by the free alkali contained in the blood, and the gradual formation of chloroform. This theory of the *modus operandi* of chloral has been controverted by M. Demarquay, of Paris, but has been in great measure confirmed by the more recent experiments of MM. Personne and Roussin,[a] who have shown that

[a] MM. Personne and Roussin, L'Union Medicale, Nov. 30th et Dec. 2nd, 1869.

chloral is, in the first instance, decomposed into formic acid and chloroform in the blood.

Chloral seldom produces the preliminary stage of excitement so constantly observed when chloroform is introduced into the system by inhalation; and the fact is thus accounted for by Dr. Liebreich:—"On account of there not being a sufficient amount of alkali in the blood, the formation of chloroform cannot take place, as it were, explosively; on the contrary, each molecule will take up a quantity of alkali from its immediate neighbourhood; and this process can only be finished after the blood has furnished the corresponding amount of alkali. A minimal quantity of chloroform is therefore formed in a unit of time, and acts at first on the ganglia of the cerebral hemispheres, afterwards on the ganglia of the cord, and at last on those of the heart. It is therefore, as it were, the very slowest method of administering chloroform."[a]

I now purpose to lay before the reader the observations I have made as to the physiological and therapeutic action of hydrate of chloral in the cases in which I have administered it.

Chloral is unquestionably a most powerful nervous sedative and hypnotic, but I have not found it quite as certain in its action as I had hoped from reading Liebreich's, Sir James Simpson's, and Dr. Richardson's papers; on the contrary, in some of the cases in which I tried chloral it failed in ordinary doses to produce any effect whatever. The average dose for an adult as an anodyne is from 20 to 40 grs.; usually 30 grs. acts as a full anodyne, but I have found 10 sufficient, and I have given as much as 80 grs. I would not, however, recommend the latter quantity to be rashly tried, as chloral is no mere placebo which, if it does no good, at least can do no harm; on the contrary, it is a substance which requires to be administered with great caution, being capable of acting with extraordinary power on the ganglionic and nervous system generally. Dr. Richardson states that 2 grs. of hydrate of chloral are equivalent in physiological value to 4 grs. of chloroform; and when we bear this in mind we cannot be surprised at the cases recorded by Dr. Noir in the *Gazette des Hôpitaux*, December 22nd, 1869, in which 75 grs. of chloral produced symptoms of a very grave and alarming character.

[a] Dr. Oscar Liebreich, "On Hydrate of Chloral, a new Hypnotic and Anesthetic." Berlin: 1869. See also Dr. Dobell's Report on the Progress of Practical Medicine, p. 227. London: 1870.

The case in which I gave 80 grs. of chloral, as will be seen when I come to refer to it presently (see Case VII), was of a very exceptional nature, and one in which I had very cautiously given the medicine in gradually increasing quantities from a very small to a very large dose. I have frequently found 10 grs. of chloral quite sufficient to cause the almost immediate cessation of violent uterine pain, and sometimes even to produce some three or four hours' sound sleep in cases which had resisted full opiates.

One of the great advantages which chloral possesses over every other hypnotic and nervous sedative is the rapidity of its action. In all the cases in which I have tried chloral, if it had any effect whatever, it was manifested within an hour of its administration; this was the furthest limit, but in the great majority of cases it acted within twenty minutes, and in several cases I have seen sleep produced or pain completely relieved in less than five minutes from the time the dose of chloral was taken.

It has been remarked by some observers that this drug differs from opium and other narcotics in the fact that the same dose continues to produce the same effect for long periods of constant use. This has not been my experience; for in several cases in which I continued the chloral for some time I found it necessary to increase the dose, and that after some time that even large doses failed in their effect.

Dr. Richardson states that the administration of chloral reduces the animal temperature in ordinary narcotism at the minimum 3°, and at the maximum 4°; any decrease of temperature below the latter figure being, according to this observer, an indication that the chloral has been pushed to a dangerous extent.[a] My experiments, as far as they have gone, have not confirmed this observation. In some cases I have noted a diminution of temperature to the extent of from $\frac{1}{5}$ to $\frac{1}{3}$ of a degree produced by the chloral; but in quite as many cases the medicine had no effect whatever on the patient's temperature. I have not, however, pursued my inquiry on this point sufficiently far to enable me to speak with any certainty on it.

The muscular relaxation which, as Dr. Richardson points out, is produced by chloral, indicates the probable value which the surgeon will doubtless derive from this agent in cases in which it is necessary to overcome muscular spasmodic action; and to the midwifery practitioner Dr. Richardson's hint should be of even

[a] Medical Times and Gazette, Nov. 6th, 1869, p. 567.

greater value than to the surgeon; for, as Sir James Simpson has shown, parturition has come on without any diminution of the parturient force in a patient under the influence of chloral. We have here the hint of a remedy probably of great power afforded us in the treatment of a class of cases in which I believe chloral was never thought of until I tried its effects in the wards of the lying-in hospital—I mean in cases of difficult labour from rigidity of the os uteri and soft parts. I shall now, without further preface, proceed to give the details of some cases in which I employed chloral

Case I.—*Cancer of the Bladder.*—February 6th, 1870.—K. C., aged twenty, an unmarried girl, who had been admitted to No. 10 ward August 5th, 1869. She had previously been under treatment in Dr. Stevens' Hospital; and when admitted into our chronic ward was suffering from acute pain in the vesical region and incontinence of urine, being unable to retain her urine for more than a few minutes together. The urine was very acid and turbid, containing muco-purulent matter and epithelium; her tongue was exceeding foul; her appetite very poor; her aspect cachetic, not hysterical; and her sleep broken by the incessant pain and necessity for frequent micturition. She was treated as the symptoms rendered necessary, and by various plans in succession, but with the same unsatisfactory result in all—namely, that under each she would generally appear to improve for a short time, and then as certainly relapse again. She continued in this condition for six months, when, the full opiates which she was now obliged to take twice a day to allay the pain, failed in its effects, it was resolved to try chloral, for the first time in this hospital; and accordingly on the 6th of February she was ordered—

℞.—Hydratis chlorali, gr. xx.
Mel. despumat, ʒi.
Aquæ cinnamoni, ʒi.
℥.—Fiat haustus statim sumendus

Ten minutes before getting this draught she passed water. Just before it was administered at 11·30 a.m, she had intense pain in the bladder, as usual; her pulse was 110, but rather irregular, temperature normal, and pupils natural; she had also a severe headache, which she had complained of for several days. Three minutes after taking the draught she had no pain whatever; ten minutes after—at 11·40 a.m.—her pulse was 104. At 11·50 her pulse was 100, pupils slightly contracted; at 12 noon pulse 96, is

dozing; 12·30, pulse 96, sleeping; 12·40 p.m., woke up for a minute, got up and passed a small quantity of urine. She states that the micturition was not as painful as usual; the time she retained her water was 80 minutes, by far the longest period she had done so since she came into the hospital. At 1 p.m. still asleep; pulse 96. She slept till 2 o'clock, when she awoke, free from pain and no headache. She says she has not had such an interval of complete ease since her admission. At 3 p.m. the pain and frequent micturition returned, though the pain is less severe than usual. At 8·50 p.m., has some pain, no headache; pulse 92; temperature 100⅓, second dose of 20 grs. of chloral given. In ten minutes the pain had ceased, and her pulse had fallen to 88. She did not sleep this night, though she had no pain whatever.

February 7th.—She had 20 grs. of chloral at the morning visit, and slept for three hours after it. At 5 p.m., being in a state of great suffering, rolling on the ground with intense pain in the bladder, 30 grs. of chloral in a draught was given. This, however, only gave her a few minutes' ease, for at 6 p.m. I found her again in great pain, and gave her 35 grs. of chloral.

February 8th.—She tells me the last draught gave her more ease than anything she had yet taken; a few minutes after it she fell asleep, and slept between four and five hours, but awoke in great pain again. At 10·55 a.m. she got 30 grs. of chloral; immediately before taking it was in great pain; countenance pale and anxious; pupils contracted; tongue furred and aphthous; skin cold and clammy; pulse about 88, irregular, weak, and fluttering. Immediately afterwards her pulse became a little stronger and more regular, and the pain appeared to cease instantly. Twenty minutes after is falling asleep; complains of cold; is free from pain; pulse 84. She slept till 2 p.m., awoke crying and nervous; not in pain; appeared stupified by medicine; has not eaten nor taken her wine to-day; has only had to pass water since 11 a.m. At 8 p.m. I was sent for, and finding her again crying with the intense pain in the bladder, gave another draught with 36 grs. of chloral, which, in four minutes, was followed by complete cessation of pain in the bladder, though she still complained much of what she describes as "a queer boiling sensation or pain in the head," over the frontal region. At midnight it was again necessary to repeat the chloral, after which she passed a good night, and was free from pain next morning. The chloral was not continued in this case.

Case II.—*Odontalgia—Insomnia.*—February 27th.—E. M'D., aged thirty, a woman two days previously confined of her sixth child; was in great pain from violent toothache, which prevented her from sleeping since her confinement. She was in a semi-febrile condition from the pain; her pulse about 100; I gave her 15 grs. of chloral in a draught; this gave her immediate ease, and within a few minutes produced sleep, which lasted for three hours. She awoke talking incoherently for a few moments, but continued free from pain.

Case III.—*Puerperal Mania.*—February 6th, 1870.—An unmarried primipara, who had been delivered five days previously after a natural labour, suddenly became maniacal. As she was being dressed, to lie outside the bedclothes, as is usual, she began to shriek and clap her hands together in a hysterical manner; and after a few moments of violent excitement, became taciturn and refused to speak a word. I was sent for at once, and ordered a draught containing 20 grs. of chloral, which produced no apparent effect. She also got wine and beef-tea. Next day at noon she had an epileptic fit, and fifteen minutes afterwards, finding her very restless, excited, and loquacious, I gave her another draught with 20 grs. of chloral. Her pulse was then 100, and she complained of severe headache. After taking this draught she immediately fell asleep and remained so for four hours, when she awoke in a stupified condition; eyes wild and staring; would not speak. She had wine and beef-tea at short intervals.

February 8th.—Her condition is nowise improved. She continues obstinately silent; pulse small and very rapid; decubitus dorsal; features pinched, and her expression fatuous. Twenty grains of chloral given at the morning visit produced no effect, and at night she had 30 grs. of chloral in an enema, with brandy and beef-tea, but only slept for one hour after getting this.

February 9th.—No change in condition or treatment. She slept that night till 4 a.m.

February 10th.—She has passed from a state of melancholia and taciturnity into one of maniacal excitement and extreme loquacity. At the morning visit I gave her (as I was visiting for Dr. Johnston) gr. xxx. of chloral, which at once subdued the excitement and gave her four hours' sleep, and at night I repeated this dose with the effect of giving her, for the first time since her illness, a whole night of undisturbed repose. Next day she

appeared to be semi-stupified, as if by the medicine; was very depressed and quiet in manner, but refused to speak a word or to take any nourishment or medicine by the mouth, and that day was sent to the Richmond Lunatic Asylum.

Case IV.—*Insomnia—Threatened Puerperal Mania.*—March 30th.—Ward No. 12.—A woman, aged about forty, unmarried, was delivered of her second child after a natural labour at 10 a.m. She was in great distress of mind; did not sleep for three nights; talked wildly; got out of bed and insisted on going home the day after her confinement; manifested an aversion to the child. On the evening of April 21st she got 20 grs. of chloral, and half an hour after fell asleep, and slept for the first night since her admission into hospital, three days before delivery. Next morning she was much better.

Case V.—*Puerperal Mania*—February 16th.—A. S., aged thirty-five—ward ; incipient puerperal mania and sleeplessness four days after delivery; was given 30 grs. of chloral at 10 a.m., slept for three hours. At 8 p.m. she had a similar dose of chloral. During the night of the 18th she had an acute attack of puerperal mania, and attempted to kill her child. This was at first treated by bromide of potassium; but this failing, on the evening of the 20th she got 30 grs. of chloral; an hour after which she fell asleep, and had nearly four hours' rest. On the 21st the puerperal mania had subsided.

Case VI.—*Puerperal Mania.*—March 5th.—I was called, in consultation with Dr. O'Neil, of Earl-street, to see a lady, aged twenty-one, who had been, fourteen days previously, confined of her first child, and had afterwards progressed favourably till the tenth day after delivery, when she was suddenly attacked by acute puerperal mania of a very violent type. When I saw her she had not slept for six nights; was highly delirious; pulse 140, and weak, as she refused to take food. Two drachms of hydrate of chloral, with the same quantity of bromide of potassi, and ʒiv. of tinct. of hyosyamus in a six oz. mixture, an ounce of which was to be given every hour, was prescribed. Next day we found her worse; she had spit out the medicine, and had not slept nor took food since. We now gave her enemata of brandy, beef-tea, and 30 grs. of chloral in each, every third hour.

The third enema produced immediate effect, and she had uninterrupted sleep after taking it. The mania, however, still continued, and we found her no better the following day. The chloral was continued till the 9th, when, finding the mania unabated, though she now slept well each night, we agreed to discontinue it, and the following day she was sent to a lunatic asylum.

Case VII.—*Hysterical Mania.*—March 2nd, 1870.—Ward 11, bed 123.—A patient, aged about eighteen, who had been in the hospital for some days suffering from hysteria, was attacked by violent hysterical mania, which came on quite suddenly this evening about seven o'clock. An hour after the attack I was sent for, and on visiting the ward I found her in such a state of violent hysterical excitement that at the urgent request of the ward nurse and patients I removed her to another ward. Having before given her chloral in smaller doses, I now prescribed:—

℞.—Hydratis chlorali, grs. lxxv.
Syrupi, ʒii.
Aquæ ad., ʒii.
M.—Fiat haustus statim sumend.

8·25 p.m.—Chloral given. She declared that nothing should induce her to go to bed. With a view of keeping herself awake she paced up and down, and running on meanwhile, as the note made by the pupil who administered the medicine (Mr. E. B. Roche) states, "against the hospital and things in general." After about ten minutes it was noticed that she was walking unsteadily, and in the next few minutes she evidently could not "toe a line." She then began actually to stagger; and as the situation appeared to be suggestive, began to talk of another occasion when she was drunk. She then got into the "maudlin stage," deplored her folly, asked forgiveness; and by taking proper advantage of this state of mind, was induced to return to the much-abused bed. She soon became calm, and slept from 10·30 p.m. till 4 a.m., when she awoke quietly.

March 3rd, 10 a.m.—She wished to go home; and on being advised to remain in the hospital, became again very much excited with hysterical crying, and was ordered 80 grs. of chloral, which was given at mid-day. Her pulse was then 100. At her urgent request her mother was sent for to fetch her home, and on her arrival she insisted on going. She walked about the ward, said the draught should not set her to sleep, and got into a state

of extreme nervous excitement about going home. Gradually, however, she began to reel and stagger in her gait, and protesting that she would not lie down, easily allowed me to put her to bed. Ten minutes after the chloral her pulse had fallen to 88; her pupils were widely dilated; talking in a thick incoherent manner, but much less excited. 12·35 p.m.—Still awake, but very sleepy; can hardly keep her eyes open, though she is trying hard to keep herself awake. 12·40.—Fell asleep, and slept till 4·15 p.m.; very calm, and awoke much more composed. 5 p.m.—She insisted on her mother taking her home; is rational, but very sleepy, and staggers so much that she cannot walk across the ward unaided; appears like a patient suffering from progressive loco-motor ataxy, having no power of co-ordinating her movements. She went home that evening. Some days afterwards she again came under my care, suffering from well-marked hysteria.

Case VIII.—*Delirium Tremens.*—February 10th.—I was sent for to see a young man who had been drinking hard for several days, but had stopped drinking three days ago and did not sleep since. He was in a state of great nervous excitement; his face pale and anxious; skin cool and moist; pulse quick, small, and compressible; tongue furred and tremulous. His wife informed me that she, her servant, and two friends had passed the last couple of nights keeping him in bed, from which he was endeavouring to escape. He had taken no nourishment whatever for three days; complains of great thirst, which he has been striving to slake by copious draughts of cold water, vomited as soon as swallowed. Ordered milk and soda water in small quantities; and the following draught:—

℞.—Hydratis chlorali, grs. xxx.
Potassi bromidi, grs. xxx.
Aquæ camphoræ ad., ʒii.
M.—Fiat haust.

This was given at 11 p.m., when his pulse was 116. In 15 minutes his pulse continued as before; in 30 minutes pulse 102—fell asleep. He slept all night; awoke more tranquil, and took some soup. Next night he had 20 grs. of chloral; slept well, and rapidly convalesced.

Case IX.—*Insomnia—Tonsillitis.*—February 13th, 1870.—A. N., aged twenty-eight—ward 10—was suffering from anteflexion of the uterus, complicated by acute tonsillitis. Not having slept for

three nights, was ordered 30 grs. of chloral, with a little syrup in three drachms of cinnamon water, as she could not swallow a larger draught. This was administered at 8·15 p.m.; her pulse being then one hundred; twenty minutes after there was no change in either pulse or temperature. She had become very excited—almost as much so as if in the excitement stage of anesthesia by chloroform. She had to be held in bed; talked loudly and incoherently; passed a sleepless night, and next morning complained of headache.

Case X.—*Acute Pelvic Cellulitis.*—February 23rd.—M. D., aged twenty-four—ward No. 10—a patient suffering from acute pelvic cellulitis. Being kept awake for some nights by the pain, was ordered 30 grs. of chloral at bed-time; this produced no effect, and she passed a sleepless night.

Case XI.—*Cauliflower Cancer from Os Uteri.*—February 16th.—J. K., aged forty-three, suffering from a large cancerous cauliflower growth from the cervix uteri, which gives rise to intense uterine pain, controlled by opiates. Every night was given 30 grs. of chloral; at mid-day, when in an exacerbation of pain, this produced immediate relief, and she had three hours' sleep afterwards.

Case XII.—*Irritable Bladder.*—February 23rd.—K. Q., unmarried, aged twenty-eight, a patient suffering from incontinency of urine; is unable to retain her water for more than ten minutes continuously. She hardly sleeps at all, being so frequently obliged to rise to pass water. There is considerable pain and soreness in the urethra and wall of the bladder. The quantity of urine voided at one time is, of course, exceedingly small. She had been previously for some time under treatment, without appearing to derive benefit. With a view of lessening the irritability of the urinary mucous membrane, and diminishing the spasmodic action of the bladder, I determined to try the effects of repeated small doses of chloral, and order a mixture containing 10 grs. to the dose, to be taken every hour. This was of decided use. The irritability of the bladder was greatly lessened. She could keep her water for two or three hours continuously, and slept soundly at night whilst taking the chloral. When it was left off all the symptoms again returned, and were not relieved until we again had recourse to it. There has not, however, I fear, any permanent improvement been effected by the chloral in this case.

Case XIII.—*Incontinence of Urine.*—February 27th.—M. J., aged twenty-eight, married; has three children; was last delivered in Glasgow three years ago, after a hard labour, and for the last two years has been suffering from incontinence of urine. This is especially troublesome at night; each morning she finds her bed saturated with urine; and, as she told me, her life had become a perfect burden to her in consequence of this infirmity. She complains much of bearing down pain in the back and hips, and also in the vagina. On examination there was no uterine displacement found, and the bladder and urethra were apparently in their normal condition. She was put on 10 gr. doses of chloral, given every third hour, with the effect of completely relieving the incontinence of urine, and allowing her to retain her water all night as long as the medicine was continued, but no longer.

Case XIV.—*Irritable Bladder.*—April 10th.—Ward No. 10.—A girl suffering from an irritable bladder, with acute pain in the vesical region, was ordered five grains of chloral at 10·35. The patient was then suffering a good deal of pain. Pulse 104.

At 11 o'clock the medicine was administered. In twenty-five minutes the pain was found to be somewhat lessened; the pulse was 96; and although the girl did not lie down she showed unmistakable signs of sleepiness.

Another dose was given, which completely removed the pain, causing decided drowsiness, with some headache.

Case XV.—*Hysterical Peritonitis.*—K. S., aged eighteen, married, was admitted to 10 ward, February 23rd, 1870. Four months ago she received a kick in the abdomen, and since then she has never been free from abdominal pain. This has been most acute for the last three weeks, and has confined her to bed. For the last three months her appetite has been very bad, and she acknowledges that she has lived chiefly on brandy and whiskey during this time. When she came to the dispensary she appeared half mad with the abdominal pain; could not stand nor answer my questions, and she repeatedly assured me that she had not slept for the last three weeks. The abdomen was small and contracted, the muscles tense and rigid, the lips drawn up, and such extreme tenderness as to render an examination difficult. She was ordered fomentations, poultices, and a draught with 35 grs. of chloral at once, to be followed by a castor oil draught with

tincture of hyosyamus in four hours. At 11·25 a.m. she got the chloral, her pulse being then 68, soft and compressible; temperature in axilla 99·8; six minutes after taking draught feels sleepy; abdominal pain still continues; pulse and temperature unchanged. At 11·55 a.m. she fell asleep, and slept soundly till 4·40 p.m. Her pulse was then 68. Soon after waking she became very excited; talked incoherently; got sick, and retched a good deal. At 9·40 p.m., as she was again complaining loudly of the extreme abdominal pain and pain in breast, with great tenderness, especially in the supra-pubic region, her pulse 72, temperature 100, pupils normal, tongue clear, but very red, she got a draught with 45 grs. of chloral in 2 oz. of cinnamon water. She objected thoroughly to the acid taste of the medicine. Two minutes after taking it pain in abdomen greatly relieved; twenty minutes afterwards pulse 78, and softer; temperature had fallen 0·5; pupils widely dilated; is very flushed, excited, and talkative. She then soon fell asleep, and slept till 3½ a.m., when she awoke complaining of the pain as before.

February 24th.—She was still complaining of great pain at the morning visit. She was ordered a turpentine draught with acetum opii; and in the evening, as the pain and restlessness continued unabated, I ordered 55 grs. of chloral in a draught. By some mistake she did not take this. I found next morning that she had contrived to smuggle in some whiskey, after taking which she slept as well as after the medicine.

25th.—Her condition was much the same as the previous days, and she was now ordered 65 grs. of chloral in ʒii. of peppermint water. She got sick immediately after swallowing this; however, she fell asleep half an hour afterwards. At 8 p m. she was in great pain and begged loudly for an anodyne. I resolved to try the effect of a larger dose of chloral than I had yet administered, as I gradually increased the dose day by day up to the present. I prescribed—

℞.—Hydratis chlorali, grs. lxx.
Syrupi, ʒii.
Aquæ cinnamoni, ʒi.
M.—Fiat haust.

Immediately before taking this, appeared in much pain, and manifested extreme tenderness over uterus. Pulse 116; temperature normal. Five minutes after taking the chloral began to talk in an excited and incoherent manner; begged me to cut her open

and see what was wrong with her. Ten minutes after she fell sound asleep. Pulse 112; respiration slightly stertorous. Thirteen minutes after medicine, pulse 100. At ten o'clock I found her fast asleep; respiration perfectly natural; pulse 80. At midnight she awoke, went over to the fire, returned to bed, fell asleep immediately, and remained so till past 6 a.m.

February 27th.—She is complaining just as before; and, in order to try whether the relief she stated she had derived from the chloral was really the effect of the medicine or not, with Dr. Johnston's approval, after the same formalities of taking her pulse, temperature, &c., with which she was familiarized when getting the chloral, a draught of sweetened peppermint water was administered.

This, however, produced no effect whatever; and at three o'clock, when I visited the ward, finding her in a state of great suffering, I resolved to try the effect of repeated small doses of chloral, and accordingly directed:—

℞.—Chlorali hydratis, ʒi.
Syrupi, ʒi.
Aquæ menthæ pip., ʒv.
M.—Fiat mistura cujus sumat, ʒi. quaqua hora.

until the pain ceases or she sleeps. The first dose produced sleep; and each time the pain returned it was repeated with the same result.

Case XVI.—*Hysterical Peritonitis.*—M. C., aged twenty-five, a patient in No. 11 ward, suffering from hysteria. At 11 a.m., April 20th, was apparently in a good deal of pain, referred to the abdomen. At 11·5 a.m. hydrate of chloral, grs. xxxv., in cinnamon water was given; five minutes after eye-lids drooped, appeared drowsy, and suffering less pain At 11·30 effects of chloral seemed wearing off, as shown by restlessness and moaning. Pulse 116; temperature 103·8 (at 10·30 pulse 124; temperature 103·1). At 11·45 fell asleep, and continued so until 12·10 p.m.

Case XVII.—*Rigidity of the Os Uteri during Labour.*—February 28th.—A primipara, aged twenty-two; ward 2; admitted in labour; has been in labour since 10 p.m. on the 26th. On the 27th, at 11 p.m., os size of florin, thick and rigid. On the 28th, at 8·45 p.m., the os being still in the same condition, though in the meantime she had been treated in the ordinary manner, by warm baths, opiates, and solution of tartar-emetic, I resolved to try the effect of chloral, and ordered the following draught, which was given at 8·50 p.m., her pulse being then 100:—

℞.—Hydratis chlorali, grs. xl.
Mel despumat, ʒi.
Aquæ cinnamoni, ʒi.
M.—Fiat haust. statim. sumend.

At 9 p.m.—Asleep; pulse 90. 9·10 p.m.—Asleep; pulse 88. 9·30 p.m.—Asleep; pulse 88. 9·38 p.m.—Roused up for a moment, had a pain, and settled down again. 10 p.m.—Asleep, not very soundly; was examined; no change in condition of os uteri. 10·15 p.m.—Having been restless and in pain for the last quarter of an hour, was ordered a second draught with 20 grs. of chloral; in all, one drachm in one hour and twenty minutes. After taking draught she at once fell asleep again. 11 p.m.—Still sleeping soundly. At 3 a.m. she woke up, and at 4·30 a.m. she was delivered.

Case XVIII.—*Rigidity of the Os Uteri during Labour.*—March 7th.—A case of rigid os in a primipara, aged twenty-four. Strong labour had gone on for 24 hours; membranes ruptured; os thin and rigid, continuing the size of a shilling. Chloral was given in ten grain doses without any apparent effect for three hours. The warm bath was now resorted to with great benefit; she had four hours sound sleep; the os then relaxed, and delivery was easily effected with the forceps.

Case XIX.—*Labour Retarded by Rigidity of Os Uteri.*—April 9th.—I was sent for to see a lady, aged thirty, who had been for forty-eight hours in labour. It was her third confinement. On examination I found the os the size of a florin, thin and rigid; pains strong; pulse weak and rapid; patient exhausted by the protracted suffering. I ordered a warm bath, an aperient enema, some beef-tea, and a draught with 20 grs. of chloral at once. In two hours I found the os the same size and the pains much weaker. She now got another stimulating enema of salt and water, and afterwards another warm bath. She was chloroformed at her own request (as she had been another time, during her last labour in Malta), and kept under the influence of the chloroform for three hours. No advance being made I gave her a second draught with 20 grs. of chloral at 8 p.m., and this being rejected I gave a third at 9 p.m. At midnight I found the os nearly the size of a crown, but still rigid and undilatable. She had a third warm bath, which appeared to relax the rigidity more than anything else; and at 6 a.m. the os

was so far dilatable that I was enabled to deliver her with the long forceps.

Case XX.—*After-pains.*—April 16th.—M. A. L., aged twenty-six, delivered of her third child, after a natural labour, at 7 p.m.; at midnight was complaining loudly of after-pains, and by her outcry kept the other patients in the ward awake. I prescribed 15 grs. of chloral as a draught at once; three minutes after taking this the pains were relieved, but she did not sleep that night. Next evening she again complained of uterine pains, for which 20 grs. of chloral was given; this produced almost immediate sleep.

Case XXI.—*After-pains.*—April 17th.—T. N., aged twenty-two, second pregnancy, confined in the same ward as the last patient, also complaining of after-pains; had a draught with 20 grs. of chloral at 9 p.m., immediately after taking which the pains ceased and she slept all night.

Case XXII.—*Acute Ovaritis after Delivery.*—April 17th.—Puerperal patient, two days after delivery complained of great abdominal tenderness, especially in left iliac region; no tympany; no pain except on pressure; lochia natural; pulse 104; tongue moist and clean; not having slept since the 14th, was given two drachms of Long's aromatized chloral, being equivalent to 20 grs. of chloral, at 11·30 p.m. Eight minutes after taking this she was asleep and slept for three hours. Next morning the tenderness returned, and her pulse was 136. She is still under treatment.

Case XXIII.—*Insomnia.*—February 28th.—A. G., aged twenty-six, suffering from debility after confinement and hemorrhoids. A very nervous excitable woman, unmarried, in a very despondent frame of mind. On the nights of the 25th and 26th slept very poorly; last night did not sleep at all. When she doses is awakened by fearful dreams; manner abrupt and peculiar; appears not far from being insane; tongue dry and covered with white fur; pulse 104; countenance pale and anxious. Complains of great headache and palpitation.

℞.—Chlorali hydratis, ʒi.
Tincturæ valerianæ ammoniat., ʒiii.
Infusi valerianæ ad., ʒvi.
M.—ʒi. 2 da. quaqua hora.

March 1st.—After taking the second dose of the mixture she went to sleep; passed a good night, and awoke decidedly better. This medicine was then continued for some days with marked benefit.

Case XXIV.—*Insomnia.*—April 19th.—Patient 125.—Ward 11.—Had not slept for two nights. At 12·5 p.m. two drachms of the syrup of the hydrate of chloral (20 grs.) were administered in water. The patient lively and talkative, was not suffering any pain; pulse 95. Within one minute after swallowing the draught her eyelids drooped, and she said she felt drowsy. At 12·10, *i.e.*, in five minutes, she was sound asleep, breathing quietly and naturally. Awoke as from natural sleep; no headache or other unpleasant symptom.

Case XXV.—*Insomnia.*—April 19th.—Patient 124.—Ward 11.—A patient suffering from insomnia and cellulitis after delivery; in place of usual anodynes a similar draught to the above was given at midnight. Draught almost at once ejected from stomach. It, however, had time to be, at least, partially taken into the system, for this patient, whose pulse was 120, and who complained of much pain, was asleep in a few minutes, and slept until morning.

Case XXV.—*Ulceration and Sub-acute Inflammation of the Uterus.*—Mrs. B., aged twenty-eight, married ten years; has three children; the youngest of them is eight years old. For the last four months she has been under my care, suffering from ulceration of the os uteri, sub-acute inflammation of the uterus, and occasional sudden brief successions of intense pain, referred to the region of the left ovary. Before she consulted me she had been under the care of an eminent London physician, and before that had been treated by a military surgeon at Netley. I need not pursue the history of her case further than to say, that under appropriate treatment the ulceration was cured, and the other uterine symptoms subsided. But the neuralgic paroxysms of ovarian pain continued to recur every ten or twelve days, and harassed her for two or three days together. Various plans of treatment were resorted to ineffectually, and she was ultimately directed to take $\frac{1}{16}$ of a grain of perchloride of mercury in 3i. of compound tincture of bark three times a day, and equal parts of the unguentum opii, hydrapyri, and iodi were directed to be rubbed in over the seat of pain. Whilst pursuing this treatment she came to me on February 18th in such a paroxysm of pain that she could not stand straight, being bent like a very old woman, to relieve the pain; her pulse 120, and her countenance expressive of great suffering. I sent for some chloral, and gave her 20 grs.; this

produced almost instantaneous relief; her pulse fell to 90; her face, from being pale and anxious, became rosy and cheerful, and she was enabled to walk home. I repeated this treatment on two subsequent occasions with similar effect.

Besides the foregoing, I have given chloral in twelve other cases; but the space at my disposal does not permit me to give the details of them in the present communication. One was a case of slight *delirium tremens*, a middle-aged gentleman, cured by 10 gr. doses of chloral, repeated every third hour till sleep was obtained. One was a case of insomnia, acute gastritis, and some mental disturbance occurring in a patient ten days before her confinement, and suffering from the abuse of wine and ardent spirits, of which she consumed an almost incredible quantity. In this case, which I saw with my friend Dr. Moore, of Amiens-street, chloral, given in large doses, failed to produce sleep, which followed the substitution of bromide of potassium for the chloral. Two were cases of irritable bladder. Two cases of rigidity of the os uteri during labour, in which chloral appeared of some use. Two were cases of puerperal mania, in one of which 15 gr. doses of chloral produced sleep; in the other 35 gr. doses had no effect; and lastly, four were cases of after-pains, relieved by from 5 to 20 gr. doses of chloral.

In some of the foregoing cases chloral failed to produce sleep; in two or three cases it not only failed to produce sleep, but, on the contrary, induced a condition of extreme nervous excitement and hyperesthesia. But in the great majority of cases the chloral allayed nervous excitement, relieved pain, or produced sleep with great certainty and remarkable rapidity. I have, moreover, observed the fact that the sleep produced by chloral more closely resembles natural sleep than that induced by opium or any other narcotics; the patients were generally free from any unpleasant after-effects, such as those produced by opiates, and informed me that they had slept pleasantly, as well as soundly, and awoke refreshed by their night's repose.

In conclusion, I may venture to say, speaking from my own experience of this substance, that hydrate of chloral is one of the most valuable hypnotics and nervous sedatives at the command of the physician. And I might venture to hazard the opinion that ere very long hydrate of chloral will come to be tried in every case of insomnia, from whatever cause it may arise, and in every case of

acute nervous pain and hyperesthesia, in which opium or other narcotics and sedatives have either failed to produce sleep or allay pain, or are contra-indicated by any cause.

ART. XVI.—*Postscript to a Case of "Diffuse Hypertrophy of the Mammæ."* By S. M. MACSWINEY, M.D., Physician to Jervis-street Hospital.

IN the Journal of November, 1869, I related the medical history of a young woman, an hospital patient of mine, who was the subject of great hypertrophy of the mammæ; and for whom my colleague, Mr. Stapleton, under whose care for surgical treatment I had placed her, removed the right breast by excision, after all other means we had recourse to had totally failed to reduce the organ in size. In that communication I mentioned that removal of the breast at the time had become necessary to save the patient's life, and I explained the grounds upon which mainly was based the decision to amputate but one breast, in the first instance at least—the expectation, namely, that this proceeding might be followed by a permanent reduction in the size of the other enlarged gland, as had heretofore happened in several examples of tumors of a like nature (and where but one breast had been excised) recorded in the annals of medicine. I further related in that paper how, after the operation, the remaining organ had greatly diminished in size; and that I even entertained sanguine hopes that it would ultimately assume a normal and permanently quiescent state; but at the same time I fully recognized the possibility of a different result; and in fact, I made the following observations:—"Should this sanguine expectation, however, not be realized, as is quite possible, and should, on the contrary, the left breast—instead of returning to, and remaining of its natural size—resume the process of abnormal growth at present suspended in it, then in this event, I will not hesitate to advise *its* removal also."

At the time when I sent the report of the case to the Journal, the process of involution of the left mamma was still actively going on. This action, however, shortly after this period, quite suddenly ceased; the size of the organ remained stationary for some weeks, then it began to enlarge again, and soon reached a dimension fully equal to what it had previously attained.

During this time both Mr. Stapleton and I frequently saw this

girl, who had returned home, and some internal remedies, and external applications were prescribed for her, but they did not exercise the least influence in retarding the process of growth, which now again became very rapid. Her health, concurrently with the renewal of the action of hypertrophy, once more failed her, and ultimately she was again received, greatly broken down in strength, into hospital, on 5th January, 1870. I measured the gland on this day, and the dimensions were as follows, viz.:—circumference, 30 inches; length, 22 inches.

The surface of the organ was unhealthy-looking; dark red in colour, and very tender to the touch. At one point the skin had given way, and a sero-sanguineous fluid was being discharged in small quantity. It was plain to see that a process of low, inflammatory action was taking place in the gland, which might, unless carefully watched and treated, end in some serious disorganization. The girl's pulse was quick; her tongue was loaded with a brownish yellow coating; her appetite was bad; and she complained of feeling weak, and very sick.

Anxious as Mr. Stapleton was to afford the poor girl the great relief, which removing the enormous mass of enlarged gland which was wearing her life away would, it was almost certain, give her, he nevertheless felt himself constrained to wait until, by rest and suitable treatment, the local disturbance should be subdued and quieted, and the general health somewhat restored.

It required some considerable time to accomplish this; but at the expiration of about a month, her condition was so much improved, that it was decided not to postpone the operation any longer.

Accordingly, on 10th February, the girl was put under the influence of chloroform, and Mr. Stapleton removed the breast in an unusually rapid manner, and almost without the loss of any blood. The breast, put in the scales, was found to weigh 11 lbs.

The following notes of the case, after operation, were taken by Mr. D. M'C. Ross, my clinical clerk, who watched the patient's progress with great interest and care:—

Immediately after the operation, she got a draught containing liq. morphia hydrochl. m. xx.; acid hydrocyanici dil. m. i. 7 p.m., same day; her pulse was 94, and full; felt chilly; vomited, which relieved her; has no headache. 9·30 p.m., pulse 108, and full; did not experience another chill, nor did she vomit a second time. Has been getting chicken broth, ice, &c., and now requests a little lemon juice, as she feels thirsty.

11th February.—Pulse 128; did not sleep very well during the night, but is sleeping in the morning; no headache or vomiting; no hemorrhage; was ordered soda in effervescence with 20 min. liq. morph. hydrocl., and 1 min. acid hydrocyanici dil. in each dose, every third or fourth hour.

12th February.—Pulse 128; tongue coated with a white fur; complains of great pain all down her left side, which she says prevents her from breathing.

13th February.—Pulse 112; tongue not so much coated to-day; has still the pain in the side; Dr. Stapleton removed all the dressing except two pieces of plaster, which had drawn the edges of the wound together; there was not much discharge; it was re-dressed with lint soaked in port wine; allowed to get some chicken and wine, and to continue her sedative mixture.

14th February.—Pulse 94; tongue cleaner to-day; not much discharge, and what there was looked healthier than yesterday; re-dressed the same as on the previous day; ordered an iron mixture, but at the same time to continue the sedative effervescing one occasionally; same diet.

15th February.—Pulse 88; tongue perfectly clean; to-day the wound was dressed in the same way; a part of the incision has healed.

16th February.—Pulse 80; tongue clean; sleeps well; no change in the general symptoms; no alteration in the diet.

17th February.—Pulse 72; sleeps well; tongue clean; same treatment.

19th February.—Pulse 68; dressed again the same way; the incision is rapidly healing.

24th February.—Pulse 68; still continuing the same dressing; is healing very rapidly; there is only a small piece to unite in the centre.

28th February.—Pulse 68; her improvement latterly has been so uniform, no lengthened remarks were necessary; the incision has now completely healed; she looks in good health; her appetite is very good; and she sleeps very well.

11th April.—On this day the patient left hospital The wound was healed; the health completely restored. She had already grown very fat, and expressed the utmost gratitude for the freedom from great suffering which the operation had afforded her.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Étude Sur Les Maladies Éteintes et Les Maladies Nouvelles. Par Charles Anglada, Professeur de Pathologie Médicale a la Facultè de Montpellier, &c. Paris: J. B. Bailliére et Fils. 1869. 8vo, pp. 646.

A Treatise on Diseases, Extinct and New. By Charles Anglada, Professor of Medical Pathology to the Faculty of Montpellier.

In the absorbing pursuit of the medicine of the present day the inheritance of the past is in danger of being forgotten. We have in a great degree neglected the counsel of our illustrious Sydenham, to study thoroughly the constitutions of epidemic diseases, and consequently have advanced but little since his time towards the solution of those difficult problems. Our literature is poor in works relating to archæological medicine, and we possess no description from the pen of a physician of any of the great pestilences of the earth, which can rival the non-professional histories of the plague of Athens by Thucydides, of the plague occurring in Justinian's reign by the eloquent Gibbon, and of the Black Death by Boccacio. We have indeed no good monograph on this subject in the English language since Mead's writings on the plague, up to the first year of the present century, when the American physician, Noah Webster, published his history of Pestilential Diseases. This can still be read with interest, and forms the text-book from which all subsequent writers largely borrow. In Bascome's work on epidemic pestilences we are disappointed in finding no references given to substantiate his statements. In Germany, the "Chronik der Seuchen," by Schnurrer, appeared in 1823, and it contains, along with much of the marvellous, a large store of valuable information. Since this came out we are much indebted to the German physicians for important works on pestilential diseases, and we need only allude to the "Morborum Antiquitates" of Gruner, to the "Mediæval Epidemics" of Hecker, and to the works of Hirsch and Haeser.

In France, until of late years, this ground has been but little cultivated, and we welcome with pleasure Dr. Anglada's interesting work. The author is Professor of Pathology to the Faculty of Montpellier, and is the son of the late Dr. Joseph Anglada, who was Professor of Medicine in the same University. The father was favourably known for his researches on toxicology, and the son has published during the last thirty years various works on subjects of general medicine. A treatise of his upon contagion appeared in 1853, and elicited the commendation of the celebrated Trousseau, who in his *Clinique Médicale* has adopted as his own the definition of Dr. Anglada, " Celle (definition) de M. Anglada, de Montpellier, me paraît mieux rendre l'idée que je m'en fais." The present work of M. Anglada is one upon a subject much contested, and is modestly called by him an étude, or inquiry, into diseases which are supposed to have become extinct, and into others which appear to be new. Though we readily concede to him the merit of originality in the *title* which he has chosen, yet the subject of his researches, in part at least, has occupied the attention of such able medical historians amongst his countrymen as Ozanam, Daremberg, and especially Littré.

The table of contents of the present volume consists of an introduction of fifty pages, in which the author discusses the general relations of his subject, and of ten chapters, in which he narrates the history of those great pestilences which desolated the earth. Like the fifteen decisive battles of the world, so vividly described by Professor Creasy, which stand forth as landmarks in history to denote the changes of empires, so do those ten plagues appear conspicuous, some of them as overturning ancient dynasties; all of them as overwhelming for the time social order, and moral and religious obligation, in one stupendous cataclysm. He heads his terrible list with the famous plague which led to the ruin of Athens in the fifth century before our era. The second chapter describes the great epidemic which occurred in the reign of the Antonines, in the second century after Christ. The third epidemic took place in the third century, and was known as the plague under Gallus. The fourth was the great bubonic plague of the sixth century, which committed such ravages in the reign of Justinian. The fifth was the great epidemic of the new eruptive fevers—small-pox, measles, and scarlatina, which dated also from the sixth century. The sixth in order was the gangrenous epidemic of the middle ages, known as " Mal des Ardents," or St. Anthony's Fire. The seventh was the

Black Death, or, as it was called in Italy, la Mortalega Grande, the Great Mortality, which raged in the fourteenth century. The eighth was la Suette Anglaise, Schweissfieber, or English Sweating Sickness, which began in the fifteenth century. The ninth plague was the great Syphilitic epidemic of the same age. The tenth and last chapter is devoted to the grand pestilence of the nineteenth century, the Asiatic cholera. However painful it may be to modern civilization to confess the fact, the last of this black catalogue of ills, as if to rebuke man's overweening confidence in his power, both in the extent of its diffusion and its great fatality, stands second to none of its predecessors.

The question to which the author addresses himself in the introduction is twofold—first, whether diseases which once were known to have prevailed have ceased to exist; and, secondly, whether new diseases have, from time to time, appeared in the world? The question is not wholly novel, for we find it proposed for serious debate some twenty centuries ago by Plutarch, who, in fact, gets up a little stage-scene to give the discussion a dramatic effect. For example, a circle of inquiring citizens is assembled in the lecture-room, the subject of the evening is given out—"Can new diseases be engendered?" The disputants are, on the one side, Plutarch in person, who appears as the advocate of freedom of opinion; on the other, a physician, who opposes the question. Plutarch argues that leprosy and hydrophobia were not known before the time of Asclépiades; the physician sticks to the good old maxim that the laws of nature are unchangeable. "What," he exclaims, and his hearers applaud him, "are we to suppose that nature acts in our bodies as men would do in a town, and that she comes out according to the fashion of the day, as an inventor of novelties?" It appears, however, that the physician is beaten off the field, for Plutarch concludes that, "without going beyond our own case, changes in the habit of living form a sufficient cause for the production of new diseases."

In fact the two hypotheses proposed to explain the origin of diseases are still open to discussion. The *realistic* theory holds that pathology is invariable, that our diseases are as old as the human race itself, though dependent on modifying circumstances for their appearance; consequently they cannot be regarded ever as being either novel or obsolete; that diseases arising from our surroundings, for instance catarrhs from atmospheric changes, are admittedly contemporaneous with our existence and never leave us;

that the specific characters which diseases evince favour the doctrine that they have originally been modelled after types, which are immutable and pre-established to act in harmony, upon which types depends that uniformity of character which is the basis of our knowledge of diseases and of the descriptions of them in our medical works. It would follow from such a view that though some diseases have appeared rarely, or but once, we are not thereby hastily to conclude them to be extinct, as judging from the uniformity of nature, and her care in economizing her materials, they may reappear, when a like concatenation of causes occurs again.

The second theory is founded upon an *a posteriori* principle, and appeals to observation and experience for support. It denies that pathology is unchangeable, and maintains that all history teaches us that certain diseases, arising out of some new cause, or combination of causes, not known to us, come up at times to the surface, like bubbles on a stream, last for a brief period, and disappear from sight and existence together. It illustrates its view by pointing to the fact that genera and species of animals and plants, now extinct, are every day disentombed from the bowels of the earth; and that consequently analogy allows us to conclude that diseases may be ranked in a similar category. Further, it holds that an unbiassed examination of the records of diseases show us new and strange forms of maladies, the very date of whose appearance can in many cases be assigned, while we ourselves are at a loss to identify some of the diseases of ancient times. The author, we need scarcely say, strenuously upholds the dogma, that diseases, like their subjects, can both be born and die.

In order to lay down clearly the terms of the question, the author premises that there are two ways by which diseases are formed.

"Les maladies se forment de deux manières, par *réaction* et par *affection*. La confusion de ces modes pathogéniques a été le vice radicale de la doctrine de Broussais."

"*Reactive* diseases are those whose first phenomenon is a morbid act, which immediately follows an injurious external impression. Their symptoms vary with the exciting agents, and correspond, with some exceptions, to the nature of the sentient impression, and to the extent of injury which has been the result of it. These diseases are bound so closely to their initiating causes, that it lies in our power to produce them at will, with a reserve for vital contingencies."

On the other hand :—

"*Affective* diseases represent a general morbid state, long in preparation, and holding in dependence eventual local determinations. Their most common origin is in obscure and undiscoverable causes, and we are not able to determine their relations with the ordinary morbid influences. Vital activity must therefore be admitted to form of itself a sufficient reason for the change which is effected in the normal condition of functions and organs. This is what we express by saying that those maladies are *spontaneous*, or referrible to some unaccustomed modification of healthy life, without the apparent concurrence of an external cause."

According to the preceding distinctions, spontaniety stands in open antagonism to external causes as a source of diseases. Still the author is forced to acknowledge the great difficulty of keeping the two distinct from all mutual interaction.

"Quand nous disons qu'une maladie est spontanée, nous ne prétendons pas affirmer, pour tous les cas et d'une manière absolue, qu'aucun facteur externe n'a pris part à sa production. Mais il reste toujours vrai qu'on chercherait vainement dans les agressions extérieures la cause prochaine des maladies affectives."

Reactive diseases must be greatly affected by differences in the civilized state. Thus, the art of war is fruitful in bringing out new kinds of fire-arms, which produce new forms of mutilation of the human body. Steam, by its introduction into industrial occupations, is not slow to create injuries of its own. Poisons, hitherto unknown, come into use, acting insidiously and injuriously upon the human frame. The imperative demand for things which were deemed luxuries a little ago, but are now become necessaries of life, keeps the working classes at an unnatural strain to bring the article to market in the shortest time, and which leads them to peril life, and regard sanitary precautions as slow. Amongst a host of examples of those new varieties of reactive disease, he cites the cases of necrosis of the maxillary bones, which occurred in the makers of lucifer matches from the vapours of phosphorus. Since the white phosphorus has been replaced by the red, this new form of disease has almost become a medical souvenir of the past.

Having laid down his two modes by which diseases are formed, the author proceeds to apply them to the explanation of the great pestilences which have produced such wide-spread havoc in the

world. Finding no external cause, or chain of causes, which could bring conviction to his mind as a sufficient reason for such prodigious effects, he feels himself logically compelled to assign these effects, by the way of exclusion, to causes working within our bodies.

A great number of spontaneous affections are *specific*. They are the work of slowly-acting causes, and present examples of the highest degree of morbid individuality. He seems to regard them as developments from the modes of living usual in societies. Gout, tuberculosis, herpetism, have had different dates of appearance in the world, and the slow elaboration of time is a necessary element for their incorporation in the human system. As M. Littré has well observed, "diseases have their chronology as well as their geography."

Dr. Anglada gives examples of geographical diseases, which are entitled to the character of novelty, as being endemic in particular countries, which possess the exclusive monopoly of them. But the fact of being indigenous to one country, would seem rather to refer their origin to obscure local causes than to purely internal elaboration.

"La Plique de Pologne, le Bouton d'Alep, le Sibbens D'Écosse, la Radézyge de Norwége, la Lèpre d'Égypte, le Pian d'Amérique, le Yaws des côtes de Guinée, le Tara de Sibérie, le Waren de Westphalie, la Fégarite d'Espagne, le Mal de la Rose des Asturies, le Ginklose d'Islande, le Noma de Suède, la Chilolace d'Irlande, représentent autant d'espèces morbides qui ne trouvent les conditions de leur développement, que dans le concours indéterminé de certaines influences topographiques." "The history of voyages and travels," he continues, "is every day enlarging the sphere of this observation, and it is no exaggeration to say that certain regions have their pathology, as they have their fauna and flora."

The influence of geographical position upon the health and diseases of the human body has been long since remarked, and may yet lead to some important discoveries upon the origin of contagions. Cicero has made the suggestive observation that our manners of life, and moral feelings, are more influenced by the *natura loci* than by family and race. "Non ingenerantur hominibus mores, tam a stirpe generis ac seminis, quam ex iis rebus quæ ab ipsâ naturâ loci, et a vitæ consuetudine, suppeditantur, quibus alimur et vivimus (*De lege Agrariâ*, cap. 35). The habits and

customs of people, the climate, mode of living, tastes, must, more or less, modify and transform both the inward and outward man.

In the list just quoted, Dr. Anglada includes as a well known indigenous disease of this country, "*la chilolace d'Irlande.*" We are not aware of any disease under this name peculiar to Ireland. On referring to Ploucquet, who has described it, we find a disease called *cheilocace, seu labrisulcium*, but it is not put down by him to our account. Rayer, in his vocabulary, describes it as a *tumefaction of the lips*, occurring in children of four or five years of age. "The lips become hard, and by separation from the teeth and gums give to the physiognomy an appearance quite unnatural. Sometimes they are divided into two parts by a deep fissure, whence flows a sanious humour. The upper lip is sometimes the only one affected, when both, the upper is the most. The English call it "the mouth's canker." We do not think this ailment to be so constant with us as to deserve the name of a national disease. We find no record of it in the works of Boate, Tuomy, Rogers; or O'Connell, and even Rayer, from whom probably Dr. Anglada borrowed it, rests its pretensions as an Irish endemic upon an "on dit."

Amongst the diseases, once prevalent, but now all but extinct, except in its retreat in the East, is leprosy. This terrible disease of the middle ages has left its mark in the numerous leper hospitals which Christian piety raised for its relief. The author states that in the thirteenth century there were 2,000 of these leproseries in France, and quotes Matthew Paris as authority for the statement, that there existed 19,000 of those institutions in Europe. This prodigious number, however, appears a mistake, arising out of the circumstance, that the Knights Hospitallers, who had the care in a great degree of the leper-houses, possessed that number of manors, "Habent hospitaliarii novemdecem (millia) maneriorum in Christianitate." These hospitals were open to other skin diseases besides leprosy, and especially to the elephantiasis of the Greeks.

In the catalogue of diseases which have become extinct are enumerated the plague of Athens, the morbus cardiacus of antiquity, the black death, and the sudor Anglicus. In that of maladies, once broadly diffused as pestilences, but now "amendeés," appear the names of scorbutus, syphilis, pellagra, and others. On the other hand, if some diseases have retired into narrower limits, on a compensating principle, others have received an increase in

extension and intensity. Diphtheria, he says, was certainly known to the ancients, but it scarcely quitted its sporadic form until the present century, when it burst like a meteor on France, Holland, and a great part of Europe. A careful examination of the medical journals would give us the accounts of strange forms of disease appearing from time to time. He mentions as such the clignotte of Guipuscoa in 1849, so called because those seized with it died in the twinkling of an eye, "dans un clin d'œil." He quotes Mackenzie for an inflammatory ophthalmia consequent on a form of bilious fever, like the febris ardens of hot countries, which, he says, "regne à Dublin." We may suggest a remarkable example of these intercurrent diseases in the recent so-called cerebro-spinal fever.

The author has laid down a practical division of epidemics into little and great. The *little* he makes to depend upon defects in our physical and moral hygiène, so that he lays upon ourselves very much of the responsibility of their origin, development, and reproduction. Perhaps this is made too much of. In Ireland, the spread of typhus fever is not altogether to be imputed to our fault, but other factors must be taken into the account. Famine, for example, is as surely with us the progenitor and associate of fever, as the λοιμος and λιμος conjoined by Hesiod. The *great* epidemics, properly so called,

"Naissent par les seules forces de la nature, c'est à dire qu'aucune combinaison humaine ne peut en préparer et en provoquer l'explosion."

He lays down a summary of the most notable marks by which we may recognize their individuality. These are, "appearance after long periods of intermission; suddenness of invasion; etiology unknown and without any appreciable relation with ordinary causes; fatality rebellious to all the efforts of art; profound specificity; aspect strange, and without anything analogous amongst known diseases." There are some diseases, such as influenza, which possess some, but want others of those marks. They are in reality common diseases gifted for a time with some accidental force of radiation, but can be referred in their source and extension to the common laws of pathology. He admits at the same time a likeness to the great epidemics in some of their less usual characters. They have, he says, in their constitution a something, he knows not what, but to which, for want of a better name, he would give the title "d'élement epidemique." He may

allow us to suggest a designation which the great Roman jurists were fond of applying to subjects in civil law, which were like the reality, but still not it. We would call them, *quasi-epidemics.*

Our author naturally asks, "d'où proviennent les grandes epidémies?" and we re-echo his question. We fear that we must be contented as yet with the "quid divinum" of Hippocrates. One of the latest hypotheses is that proposed by M. Fuster in his book on the diseases of France. This writer is of opinion that he has discovered the secret to exist "in an undetermined combination of cosmical causes, and of moral and political influences." This is, in point of fact, a revival of the doctrines of Noah Webster, who tries to prove that strange commotions in the physical world used to precede and accompany the great pestilences. M. Fuster would add great moral perturbations to these conjoined causes.

The only way to throw a ray of light upon the mysterious origin of epidemics, is to note down carefully and without bias the points in which they agree, and those in which they differ; the characters which never are absent, and those only accidentally present; the centre from whence they seem to proceed, and the barrier where they cease to spread. Also, how far their manifest preference for some places or persons, and their indifference to others, may be found to arise from caprice or from law. In all probability this is a question which we shall bequeath for its solution to posterity.

We have occupied so much of our space with the general principles of epidemic pestilences that we have not time for comment upon the several examples. We may, however, be allowed to say in general, that they form most interesting and highly important dissertations, written in a very attractive style. M. Anglada will, we trust, allow us, before we lay down his work, to clear up a slight historical error which occurs in his chapter upon the English sweating sickness, as regards this country. It is there stated that Scotland and Ireland enjoyed an immunity from this epidemic. As regards Scotland, it is supported by the authority of Dr. Freind, who says, "Quod mirum est, in exteris nationibus, Scoti illæsi, Anglique tantum tentati erant, neque in Anglia peregrini afficiebantur." We have no grounds to prove that Ireland escaped, yet Dr. Anglada, on the assertion of Larrey, states, "le reste du royaume fut frappé presque en même temps, toujours à l'exclusion de l'Ecosse, et de l'Irlande," and he repeats the observation while describing the repeated epidemic returns of the disease. It is possible that on this point the author was misled by Hecker, who

has stated positively that "the sweating sickness did not advance westward to Ireland, nor did it pass the Scottish border." "The historians who would," he says, "certainly have recorded so calamitous an event are entirely silent." Nay, further, he goes on to found a theory for its restriction to the English nation. But there is abundant proof that M. Hecker's facts and theory are alike unsustained. In Hardiman's *History of Galway*, compiled from the town annals, we are told "this charitable institution (the Leper Hospital) was fortunately completed in the year 1543, when the sweating sickness broke out and raged with great violence, destroying multitudes of the natives, and particularly the tradesmen of the town." This would seem to refer to the fifth epidemic of this plague. Again, in Smith's *History of Cork* (Vol. ii, page 35), we find it stated on the authority of local MS. that in 1528, the fourth epidemic, there was "a malignant disorder called the sweating sickness in Cork." The same fact is recorded in the *Annals of the Four Masters* under the year 1492, the first epidemic of the disease, "An unusual plague raged in Meath, a plague of twenty-four hours' duration, and any one who survived it beyond that period, recovered. It did not attack infants or little children." In the Dublin copy of the *Annals of Ulster*, this is called "plaigh allais," the sweating sickness, "that sad contagion which no former age knew."

We cannot rise from our present pleasing task without cordially thanking M. Anglada for his work, which supplies what was felt to be a desideratum in our general medical literature.

JOHN POPHAM.

WORKS ON HYGIENE AND SANITARY SCIENCE.

1. *A Manual of Practical Hygiene, prepared specially for the Medical Service of the Army.* By Edward A. Parkes, M.D., F.R.S.; Professor of Military Hygiene in the Army Medical School. Third Edition. London: Churchill and Sons. 1869. Large 8vo, 640 pages.

2. *Traité d'Hygiene Generale.* Par le Docteur Adolphe Motard. Paris: Baillière. 1868. 8vo, 2 vols, 878 and 821 pages.

3. *Traité de l'Alimentation dans ses rapports avec la Physiologie, la Pathologie et la Therapeutique.* Par le Docteur Jules Cyr. Paris: Baillière et Fils. 1869. Large 8vo, 578 pages.

4. *Disinfectants and Disinfection.* By Robert Angus Smith. Ph. D., F.R S. Edinburgh: Edmondston and Douglas. 1869. 8vo, 138 pages.

The first two editions of Professor Parkes's work on *Hygiene* have been so fully described in this Journal that the task of reviewing a third issue is a light one, more especially as its immediate predecessor was published only three years ago. It is not, however, altogether because the great demand for this work speedily exhausted the second edition that a third one so soon followed, for so zealously is sanitary science now cultivated that even the comparatively brief period of three years produced no inconsiderable amount of work "to be written up." Dr. Parkes takes care to keep himself thoroughly and intelligently posted up in reference to the labours of sanitarians throughout the world, and everything that is really worthy of record, or likely to be of the least practical use, he presents to us in his admirable work.

Of the former edition of Parkes's *Hygiene* we have written in the most commendatory terms, and of the present we can truthfully aver that it is by far the most valuable treatise on sanitary science in the English language, and that it is—taking into account its special military application—quite equal to the best treatises of the same subject to be found in France or Germany. In this work we find not only the most recent information, but every doubtful statement is critically examined and placed in such a way before the reader as to enable him to judge of its value. Indeed, as a rule, the pages of the work are not encumbered with mere theories and hypotheses, but are filled with substantial facts. If a process for purifying water, or for disinfecting foul air, is given, Dr. Parkes tells us to what extent we may rely upon its efficacy, whether or not the process is sure to give good results, and under what conditions. As a rule, writers on sanitary science are far too inexact in their figures and conclusions. They write with far too great a degree of positivism. Into this fault Dr. Parkes has not fallen. He recommends various sanitary agents, but at the same time he tells that some of them often fail, even when they are most likely to prove useful. His book is in every respect a candid one, and he claims no more for the science of which he is so zealous a cultivator than that which is indisputably due to it.

The third edition is not much larger than the first one, although it contains more information. This is accounted for by the great

condensation in the mere verbiage of the book, which distinguishes the present edition. Not one word is wasted, and in almost any fifty pages of this manual more substantial information will be found than in many a bulky work on science. In no place, however, is brevity carried so far as to render the meaning obscure. A more intelligible or more correctly worded volume we have never read.

The general arrangement of this edition is similar to that of its predecessors, but several chapters have been entirely re-written. The most striking alteration is the adoption of the new notation in chemistry, a change which, we fear, will prove somewhat embarrassing to medical readers who "took out their chemistry" from six to thirty years ago. The new notation is now almost universally in use amongst pure chemists, and it is gradually finding a place even in the chemical language employed in the medical journals. We notice, however, that Dr. Parkes uses, where he can, the common names of chemicals—for instance, oil of vitriol, or, even more learnedly, sulphuric acid, instead of sulphuric hydride.

A large proportion of Parkes's *Hygiene* is occupied with matter solely of interest to military men. We now venture to suggest to the author the desirability of preparing an edition of his excellent work adapted for the use of the civil population. About three-fourths of the existing work would be available for this purpose, and if the remaining fourth part were replaced by a few chapters on sanitary legislation, including a digest of the Sanitary Act of 1866, a work of great value to the general public would be the result.

Our French neighbours have long been distinguished for their writings on hygienic and diatetic subjects. The comprehensive works of Levy, Tardieu, Donne, Fonssegrives, and many other authors, prove how highly appreciated are the labours of sanitarians in France, and how industriously hygienic science is cultivated in that country.

M. Motard's work is of a very comprehensive character. There is hardly a subject in any way relating to public or private hygiene which is not fully discussed in it. The work is divided into seven parts, or books, each of which is subdivided into a large number of chapters. The first book is, to some extent, of an introductory character, and is the most theoretical portion of the work. A large portion of it is devoted to that most recondite and unsatisfactory subject—the origin of man. This delicate topic is, however, very carefully treated, and the author takes by no means a purely

materialistic view of the intellectual part of human nature. He justifies that artificial arrangement in the scientific classification of animals, which assigns to man a separate and independent kingdom. The rest of this book is devoted to the consideration of the anatomy and chemical constitution of man, the physiological uses of the more important organs, the influence of the physical state of the body upon the mind, and the doctrine of temperaments.

The second book treats altogether of climates, and is illustrated by numerous maps. This part of the book is so very scientific that minute instructions are given in it for the quantitative analysis of the air. If one wants to know the mean minimum or maximum temperature of the air and water in almost any part of the civilized world, the information may be readily found in this book. As might be expected, the influence of soil upon the public health is very fully dwelt upon, though we were rather surprised not to find some reference to the remarkable facts relative to the improvement of the health of English towns by thorough drainage, which have been recorded in late volumes of the Annual Reports of Dr. Simon, the medical officer of the Privy Council. Relative to potable waters, Dr. Motard writes very fully, largely quoting the results and opinions given by British chemists and physicians. He inclines to the view that most zymotic diseases are more or less circulated by means of potable water.

The third book may be regarded as a complete treatise on nutrition. It describes the nature and preparation of foods of every kind, enters elaborately into the consideration of their influence upon both the body and mind of man, and discusses at great length the uses and abuses of condiments, alcoholic liquors, and other stimulants. This book, and the two which we have already referred to, constitute a bulky volume of 876 pages, a fact which shows how exhaustively every subject has been dealt with.

The second volume consists of four books, the first of which is devoted to clothing, ablutions, and exercises; the second to labour, or the work of man in the arts of peace and war; the third to prophylactics, or the nature and prevention of zymotic and other diseases, which are to some extent under man's control; and the concluding book is wholly taken up with the social conditions of civilized communities—education, justice, police, &c.

We have preferred to give a brief statement of the whole contents of this excellent work rather than to describe any portion of it fully in detail, for it is not a book which admits of extracts. It

is, indeed, a kind of encyclopedia of sanitary science, and one which should be found in the library of every one who devoted himself to the solution of problems relating to the health and well-being of the community. It has been said of Kepler that he was a great mathematician, but a bad philosopher. We may say of M. Motard, that we hardly know in which character—that of a philosopher, or that of a mere scientific writer—he is most deserving of our commendation. A more accurate, complete, and elegantly written work of the kind has never come under our notice; and we can conscientiously say, that having carefully read it through, we were unable to find any statements which could fairly form the subject of hostile criticism.

Dr. Jules Cyr's work is a treatise on "Food and Diet," which we should be very glad to see in an English form. In some respects it resembles Dr. Moleschott's celebrated work on food and diet, of which a defective partial translation into English was made a few years ago. It is, however, a much more practical treatise than Moleschott, and one which is much more likely to prove useful to the physician.

The greater part of this work is of course devoted to the description of the chemical composition, and relative nutritive values of all the more commonly employed foods. We find also much space devoted to such subjects as the preservation of aliments, the influence of cookery upon food, &c. The more important portion of the work is, however, that which treats of the use of food in disease, and of the diseases produced by the use of unsound and diseased foods. The author speaks very favourably of the advantages resulting from the use of a milk diet. He quotes numerous authorities in support of this view; amongst others, Dr. Karell, who states that in dropsical affections, phthisis, diseases of the liver, and in diseases of the stomach and intestines, following affections of the nervous centres, he considered the milk cure as the first of remedies.

With reference to the diseases produced or induced by the use of bad water, Dr. Cyr states, that goitre and cretenism are probably the result of an excess of a particular kind of organic matter in that liquid. The hypothesis that excessive quantities of earthy (chiefly magnesia), salts are the cause of these repulsive diseases, is not yet exploded, and the arguments in favour of that hypothesis are certainly much stronger than those which, according to Dr. Cyr, should lead us to infer that organic matter in water produced goitre.

We were not a little gratified to find in Dr. Cyr's works, many references to the labours and opinions of Irish physicians, particularly to Graves "le célèbre professeur de Dublin," and the Rev. Professor Haughton.

We shall conclude this notice of recent works on hygiene by a brief reference to Dr. R. Angus Smith's little book on *Disinfection*. It consists of a few chapters, in each of which is described the preparation, mode of application, and supposed efficiency of one or more of the substances used for disinfecting purposes. The author praises sulphurous acid, nitrous acid, &c., but his perfect type of a disinfectant appears to be carbolic acid and its compounds. A large proportion of the contents of the book is taken up in describing experiments with the latter disinfectant.

Dr. Smith may be regarded as one of our best, if not the best, of our chemical authorities on disinfection, and we are therefore disposed to pay great respect to his opinions relative to the remarkable sanitary properties of carbolic acid. Our experience of that agent is most favourable, though in the case of the disinfection of the air of sick rooms, we should depend more upon the action of nitrous acid, than of carbolic acid, vapours. We think Dr. Smith might not have taken up so much of his work with mere details of experiments with carbolic acid—the results would have been sufficient. He might also have given more precise information relative to the application of the various disinfecting agents, so as to render his book of practical utility to medical officers of health, and physicians who are not skilled in chemical manipulation. Indeed Dr. Smith would have conferred a boon upon the public had he given us a cheap and handy little book on disinfection, containing plain directions for the purification of air, water, and clothing. We fear the book under our notice, excellent though it undoubtedly is, will not be largely purchased by the class for whose benefit we presume it has been written.

WORKS ON ELECTRICITY.

1. *Electricity in its Relations to Practical Medicine.* By Dr. Moritz Meyer. Translated from the third German Edition, with Notes and Additions by W. A. Hammond, M.D. New York: D. Appleton & Co. 1869. 8vo, pp. 497.
2. *A Treatise on Medical Electricity.* By Julius Althaus, M.D. Second Edition. London: Longmans, Green & Co. 1870. 8vo, pp. 676.

Of the standard works in German devoted to medical electricity, viz.: those of Remak, Rosenthal, Ziemssen, Benedikt, and Meyer, the last alone, so far as we know, has appeared in an English dress, and thus become accessible to a wider circle of readers. The Americans have, in its translation, as well as in many other instances, outstripped us in their zeal to bring the writings of the best continental authors within the reach of all. An acquaintance with the groundwork of the physics of electricity, and with the principles of electro-physiology is an essential preliminary to the rational employment of electricity in diagnosis and prognosis, or as a curative agent, and no practical physician can now in justice to himself, or in fairness to his patients, afford to neglect the help to be derived from electricity. An electrical apparatus, of one form or another, will soon become as necessary to a physician's equipment as a stethoscope or a thermometer, and as a complete guide to all who would become conversant with the present state of electro-therapeutics Dr. Meyer's treatise can be safely recommended.

The American edition leaves nothing to be desired as to its external attractions, and though we cannot say quite as much of the merits of the translation, Dr. Hammond has enriched it with some important notes, especially under the head of Infantile Paralysis, to which he has paid considerable attention.

Static electricity as represented by the ordinary plate or cylinder machine, and the Leyden jar, has for some time practically been abandoned, perhaps not quite deservedly, and the most important question now to be decided is the relative value of the direct, or constant, and the induced current.[a]

[a] The term "induced current" is not open to the ambiguity which attaches to the phrase "interrupted current," often used as synonymous with it, for it is plain that a galvanic current (*i.e.*, direct battery-current) can equally well be interrupted, and moreover induced or faradic currents do not operate by virtue of the interruption, but the essence of their mode of action is that they are *momentary* currents which quickly rise from zero to a maximum, and *vice versa*, so that a very rapid change of density is brought about.

Until a sufficiently permanent and portable battery for furnishing the direct current is provided, the superior convenience of the small and elegant induction apparatuses now made will ensure their more general adoption. But as it is clearly a fundamental point to come to a distinct understanding as to the respective functions of the induced and the direct current (the so-called constant or continuous current), we will briefly abstract some of the leading conclusions at which Dr. Meyer arrives:—

1. The *interrupted* current is applicable in those cases in which we wish (*a.*) to excite the muscles, the nerves of special sense, the sensary or motor nerves; (*b.*) to produce contractions of the blood or lymphatic vessels; (*c.*) or to affect certain organs supplied by the sympathetic.

2. The temperature and volume of a muscle are increased by the contraction produced by Faradization.

3. An increase of sensibility is effected by currents of tolerably quick intermission, and not exceeding a certain duration and intensity; under opposite conditions there is soon a marked decrease in sensibility. Hence we should only use currents of such strength and duration as are sufficient to produce the desired result.

4. If we wish to relax a tense muscle, repeated intermissions of a strong current, or single shocks from a strong induction current are generally more efficacious than the constant current.

5. The use of the *constant* current is indicated in those cases in which we wish—

(*a.*) To excite the nerves of special sense or the cutaneous nerves.

(*b.*) To destroy the skin or mucous membrane.

(*c.*) To produce an increase of warmth.

(*d.*) Or to induce a chemical process, *e.g.*, coagulation of blood.

6. While, as a rule, the induced current furnishes the most powerful means for exciting the motor nerves and muscles, there are certain peripheric paralyses in which the constant current, probably in consequence of its longer duration, produces effects which cannot be brought about by the induced current.

7. The sensitiveness of a muscle to the induced current can, in many cases, through the application of a tolerably strong constant current, be increased.

8. In galvanizing a nerve it is advisable frequently to change the direction of the current, because the conductivity for a current, flowing long in the same direction, decreases, while with a change in the direction it increases.

12. There is no doubt that we can, by means of a constant current, even of tolerable strength, affect through their bony coverings the brain and spinal cord.

The paragraphs relating to the electrotonic state are omitted. In the opening sections of the book a brief sketch is given of the history of medical electricity, and of the different forms under which electricity is manifested, followed by an outline of animal electricity. The action of the electric currents on the different organs and tissues is more fully discussed, and a detailed description is given of some of the various kinds of apparatus designed especially for therapeutical purposes.

The method of using the induced and constant current with precision is described, and a section is devoted to the application of Electricity to Anatomy, Physiology, and Pathology. Of more interest to the physician is the direction of electricity to the diagnosis and prognosis of paralytic affections, in which class of diseases it can render most important aid, and the author having thus fully treated the *principles* of electro-physiology, enters at length, and with thoroughness, into the *art* of electro-therapeutics as illustrated in medicine, surgery, and obstetrics.

The number of editions through which Dr. Meyer's work has passed in Germany attest the esteem in which it is there held, and we do not doubt that an equal measure of approbation will be accorded to it in this country.

It is with great pleasure that we welcome, and cordially recommend Dr. Althaus' treatise, in the improved guise of a second edition. Dr. Althaus' work possesses the merit of being clearly and agreeably written, while its teaching is in accord with the most recent information; and the balance is evenly held between the relative virtues of galvanization, and faradisation—a point of the highest importance in the treatment of disease. Among the more recent positive gains to medical electricity, we can reckon the right appreciation of the use, and even indispensableness of the direct current in certain cases of paralysis; the more certain cure of severe neuralgia; the undoubted possibility of directly galvanizing the brain, and spinal cord; and the galvanic irritation of the sympathetic nerve.

These as well as many other questions of great interest and importance, such, for example, as the electrotonic action of the direct current on living nerves, are carefully discussed in the work before us.

One of the subjects to which Dr. Althaus has directed special attention, and on which he is entitled to speak with authority, is the electrolytic treatment of surgical diseases. Three years ago he had already published a pamphlet on the electrolytic treatment of tumours, and shown that the proper mode of procedure was to introduce one or more needles connected with the *negative pole*, into the depth of the tumour; while a moistened conductor, *e.g.*, a sponge, was connected with the positive pole, and placed on the skin adjacent to the tumour. This method has been successfully applied to aneurism, nævus, bronchocele, sebaceous tumours, hydatid cysts of the liver, &c.; and it is probable that we do not yet know the full extent of its powers.

It is most suitable, and will, in all likelihood, find its chief use in the dispersion of tumours containing fluids, especially, serous cysts, and effusions; and we have succeeded in rapidly dispelling, by this means, a ganglion of the wrist, which had existed for a great length of time.

To the favourable opinions already accorded to the former edition of this treatise, we can add nothing, except in the way of sincere commendation; and to Dr. Althaus belongs the credit of having filled up a hiatus in English medical literature, by the production of a sound, comprehensive, and practical work, on the right use of an agent, whose powers are daily becoming better recognized, and more clearly defined.

A System of Surgery, Theoretical and Practical, in Treatises by Various Authors. Edited by T. Holmes, M.A., Cantab.; Surgeon and Lecturer on Surgery at St. George's Hospital. Second edition. London: 1870. Vols. I. and II.

In noticing the publication of the first two volumes of the second edition of *Holmes' Surgery*, we have to congratulate the editor on the success of his first edition, and on the many improvements on it which are noticeable in the present issue. The arrangement of the text has been extensively altered; many passages which were printed as notes in the former edition being introduced into the text, which has itself been revised. The first edition contained no illustrations; the present is illustrated both by coloured lithographs, and with woodcuts inserted in the text, an addition which will greatly increase the usefulness of the work. The illustrations, the

editor informs us in his preface, have been supplied as freely as was possible, "without unduly increasing the cost of the work." The paper on Inflammation, by Mr. Simon, has been materially altered in the present edition by the postponement of the discussion of the pathological theories of inflammation to the promised fifth volume of the work. The object of this change was to allow time to elapse before the consideration of questions which are at present the subject of the utmost controversy, resulting from the publication of Cohnheim's views. We hope, though our hope is far from sanguine, that the publication of the fifth volume will set at rest our uncertain ideas on this subject. Mr. Croft's paper on Hectic and Traumatic Fevers has been transferred from the fourth volume to the first, and has been illustrated by thermographs of the changes observed in these forms of fever, which will be studied with interest. The observations of Billroth, which have been repeated and confirmed by Mr. Croft, show that the highest temperature reached in cases of traumatic fever does not exceed F.104·9°, and that under favourable circumstances the fever begins to decline on or after the second day; cases which attained the maximum temperature on the third or fourth days were, with a single exception, attended by some complication. The thermographs obtained in hectic fever show well the remittent character of the fever. In these cases a temperature about F.102° is exceptional, and sudden exceptional falls and rises of temperature indicate the occurrence of complications. We look forward with anxiety to the publication of the remaining volumes of this excellent work, more particularly to the fifth, which promises to contain so much new and important matter.

The Body and its Health, with Teachers' Aid and Questions. By E. D. MAPOTHER, M.D., Professor of Physiology, Royal College of Surgeons; Surgeon to St. Vincent's Hospital, &c. Third edition. Dublin: John Falconer, 53, Upper Sackville-street.

THE favourable opinion which we formed of Dr. Mapother's book has been endorsed by the public, and already a third edition has been issued. Along with it may be had questions, which those engaged in teaching elementary physiology will find admirably suited for ascertaining how far their pupils have comprehended the lessons contained in the body of the work.

Nouveau Dictionaire de Médecine et de Chirurgie Pratiques Illustré de Figures intercalées dans le Texte. Rédigé par Benj. Anger, E. Bailly, A. M. Barrallier, Bernutz, P. Bert, Bœckel, Buignet, Cusco, Demarquay, Denucé, Desnos, Desormeaux, A. Desprès, Devilliers, Alf. Fournier, T. Gallard, H. Gintrac, Gombault, Gosselin, Alph. Guérin, A. Hardy, Heurtaux, Hirtz, Jaccoud, Jacquemet, Jeannel, Kœberlé, S. Laugier, Lannelongue, Ledentu, Liebreich, P. Lorain, Lunier, Luton, L. Marchand, A. Nélaton, A. Ollivier, Oré, Panas, M. Raynaud, Richet, Ph. Ricord, Jules Rochard (de Lorient), Z. Roussin, Saint-Germain, Ch. Sarazin, Germain Sée, Jules Simon, Sireday, Stoltz, A. Tardieu, S. Tarnier, Valette, Verjon, Aug. Voisin. Directeur de la rédaction: le docteur Jaccoud. Paris: J. B. Baillière et Fils.

The eleventh volume of this *New Dictionary of Medicine and Surgery*, in large octavo, has now appeared, each volume containing about 800 pages, and it brings us only to the end of the fourth letter of the alphabet; from this some idea may be formed of the extent of the work. To many of the articles we shall have occasion to refer from time to time, and at present, therefore, we merely draw attention to the general character of the undertaking. So far as he has gone, the editor seems to have omitted no subject connected with the practice of medicine and surgery. The reader will find discussed as fully as in the special treatises questions in anatomy, physiology, pathology, chemistry, botany, pharmacy, hygiene, state medicine, dietetics, and midwifery, as well as in practical medicine and surgery. Innumerable matters on which he would find it impossible elsewhere to obtain information, are here considered. The articles, as far as we have been able to read them, are excellently written; they are abundantly illustrated, and all the important ones are accompanied with most valuable bibliographical indices; indeed, the writers seem to have been instructed to deal exhaustively with the subjects committed to them; sometimes the wish to do this has led them to enter into matters of historical, literary, or speculative interest, with a minuteness of detail which most readers would not desire. The production of such a work as this is a gigantic enterprise; we do not know of any similar one which approaches to it in completeness; we have no hesitation in saying every medical library for reference, should possess it, and

we have little doubt it will soon be found in every public one throughout the civilized world. Every honour is due to those who have projected it, and are carrying it to completion.

Nouveaux Elements d'Anatomie Chirurgical. Par BENJAMIN ANGER, Chirurgien des Hopitaux, &c.

THE chief merit of this work lies in the illustrations of normal and morbid anatomy, which are intercalated in the text; while the volume contains 1,056 pages, the text contains 1,079 illustrations, all of which are admirably executed.

The work is divided into two sections, the first of which is devoted to general surgical anatomy; the second, to the anatomy of the regions of the body. Under the head of general anatomy, the subjects of the embryology and the anatomical elements of the blood and of the tissues are examined : the second part devoted to the consideration of regional anatomy, contains excellent representations of the normal anatomy of each region; with a clear and concise descriptive text. In both sections the description of each tissue and region, concludes with an illustrated account of the morbid anatomy of each element and structure.

In the attempt to condense so much matter into a single volume, many subjects are necessarily treated imperfectly, and many omitted entirely; but these defects are compensated by the excellence and great variety of the illustrations of those treated of more fully. Many of the omissions which we have observed, are difficult to explain; such, for instance, as the omission of any detailed description of fractures of the clavicle, or illustrations of them; while many of the corresponding injuries of the humerus are depicted and described. We infer that the author has been forced to omit many such subjects, in order to keep the work within its present compass, as we are aware that he has in another work very fully figured these injuries. Again, in the description of the surgical anatomy of the humerus, we fail to find any account of the anatomy of its epiphyses, or any but the slightest allusion to their separation by injury; none whatever of the separation of the lower epiphysis, while the diagnostic signs of fracture of the lower extremity of the adult bone, and of the luxation of the bones of the forearm backwards are considered in detail. Many such omissions occur, and necessarily diminish the value of the work as a text book; but we know of no other which contains, in the same space, so great a variety of excellent pathological drawings.

PART III.

QUARTERLY REPORTS.

REPORT ON MEDICINE.[a]

By James Cuming, M.A., M.D.; Professor of Theory and Practice of Medicine, Queen's College, Belfast; Physician to the Belfast General Hospital; Examiner in Medicine in the Queen's University in Ireland.

RECENT OBSERVATIONS REGARDING THE NATURE OF PULMONARY CONSUMPTION.

Several questions of the highest importance have been recently discussed both in this country and abroad in connexion with the subject of pulmonary consumption. Of these the one possessing most practical interest is whether phthisis is to be regarded as being always of a tubercular nature, or whether it is engendered by ordinary morbific causes independently of any specific form of lesion.

A few years ago the invariably tubercular character of phthisis was one of the most unquestioned propositions in pathology. Physicians accepted this view of the subject on the authority of Laennec as an axiom in medical science, and although the doctrines of this illustrious observer were hotly disputed in his own time, they ultimately came to be universally adopted almost without question. Within the last few years, however, very serious attacks have been made on the prevalent belief, and a large amount of clinical and pathological evidence adduced in favour of the doctrine that the etiology of phthisis cannot be summed up in the word tubercle, but that this wide spread and terrible malady may be and often is engendered in individuals who are not tuberculous, and who never become so.

That this is not a mere question of pathological theory will be at once evident. If consumption be always the result of a diathesis, and if the lung affection be only the material evidence of the dyscrasia, then the prophylaxis and management of the disease

[a] The author of this Report, anxious that every contribution to Pathology and Practical Medicine should be noticed, will be glad to receive any publications on these subjects. If sent to correspondents of the Journal they will be forwarded.

must be conducted on different principles to those which we should be obliged to adopt if the disease were likely to be brought about by a neglected cold, as is popularly believed, or by a pneumonia or hemoptysis. Accordingly it becomes a question of great significance how far we are justified in regarding the well-known symptoms and physical signs of consumption, and the destructive changes in the lung in connexion with them, as always produced by tubercle.

That pulmonary diseases exist, unconnected with tubercle, which present local and constitutional manifestations closely resembling those of phthisis, has been long known. Cirrhosis of the lung, for example, is a wasting disease attended by cough, expectoration, hemoptysis, and by physical signs very like those of phthisis. The more recently described fibroid phthisis is another non-tubercular affection of a similar character. In cases of this malady which has been well described by Dr. Sutton[a] and others, it has been found quite impossible to establish the diagnosis, from tubercular phthisis merely by stethoscopic examination. The changes in the lungs which, it is to be noted, are found especially in the upper lobes are, however, merely the result of fibroid degeneration, the newly formed fibroid tissue in some parts, completely destroying the pulmonary parenchyma. There are also several varieties of what is called mechanical bronchitis, such as the pulmonary affections produced in knife-grinders, stone-cutters, coal-miners, and in individuals pursuing several other trades, the general exciting cause being the inhalation of minute particles which act as mechanical irritants, and the symptoms and physical signs being in many instances identical with those of phthisis. Two remarkable cases recently recorded by Zenker[b] put the nature of this malady in a very clear light. In these cases the disease was caused by the inhalation of oxide of iron, this substance in fine powder being used in the manufacture of little books in which leaf gold is placed. The air of the room in which the patients worked was actually coloured by the dust of the iron, and the walls and furniture were covered with it. The lungs of two persons who had worked in this atmosphere presented a remarkably red tint, owing to the presence of the iron in them, and the lungs of one patient were found, on chemical examination, to contain the extraordinary amount of above 320 grains of this substance.

[a] Medico-Chirurgical Transactions, Vol. xliii, p. 287.

[b] Deutsches Archiv. fur Klinische Medicin, band 2, S. 116.

It had been frequently disputed whether the black discolouration occasionally met with in the lungs is owing to pigment derived from the blood, or to minute particles of carbon which had been inspired with the air, and the question had been asked by Friedreich, why it never happens that red discolouration of the lung is found. Zenker's cases remove any doubts that may have existed on this subject.

In one of these cases that of a woman, aged thirty-one years, both lungs exhibited on section a number of small nodules, some of which were altogether solid, others had a small opening in them corresponding to a very small bronchial tube. All the lobes of the lungs with the exception of the middle lobe of the right lung contained irregular cavities of various sizes.

Dr. Zenker remarks that this case would formerly have been unhesitatingly described as one of tubercular phthisis. He argues that this view is untenable, because nothing presenting the anatomical or histological characters of tubercle was found in the lungs, and no tubercles were met with in any of the other organs. The indurated nodules he regards as the result of lobular interstitial pneumonia, and the cavities as formed in consequence of an ulcerative process commencing in the bronchial mucous membrane, and after penetrating the wall of the bronchial tube involving the pulmonary parenchyma.

Dr. Greenhow[a] gives an account of the morbid appearances in several cases of lung disease arising in colliers, miners, and flax-dressers. In one of the best marked of these cases, a large irregular ragged cavity existed. The general conclusions of Dr. Greenhow are as follows:—

"The nature of the substance inhaled appears to be of secondary consequence as regards the ultimate result, excepting that the heavier and more penetrating kinds of dust, such as angular particles of grit, more speedily excite serious disease than the lighter kinds. Mechanical particles inhaled into the air-passages set up, in the first instance, more or less irritation of the bronchial membrane, which may entirely subside again if exposure to its exciting cause be discontinued. On the other hand, a continuance of the mechanical irritation rarely fails sooner or later to induce those changes in the interstitial tissue of the lungs seen in the several specimens which I have exhibited to the Society on the present and former occasions.

"The characters common to all these cases are that the pleura is

[a] Transactions of the Pathological Society of London, 1869, p. 41.

generally somewhat thickened over a greater or less extent, and that pleural adhesions are frequent. The density of the lungs is generally more or less increased, and patches of consolidation of various sizes are found in their substance. Sometimes the greater part of a lobe is so consolidated; but more frequently the consolidation is of smaller extent, and is distributed throughout the lungs in several, sometimes many, well-defined hard nodules, varying in size from a hemp-seed upwards to that of a walnut, or even larger. The consolidated tissue, whether forming small nodules or occupying the greater part of a lobe, cuts with a smooth dry section, and, unless where the orifices of divided bronchial tubes are visible, presents no resemblance to ordinary lung tissue, all traces of the vesicular structure being obliterated. On section, the nodules are sometimes of an iron-grey or greyish-yellow colour, but more commonly black, or nearly so, from the abundant deposit of pigment in their tissues; in many cases they are seen with the naked eye, and in almost all cases with the microscope, to be intersected by bands or lines of fibrous tissue. When the smaller nodules are cut across, the orifice of a divided bronchial tube is generally seen in the middle of the nodule, showing apparently that the consolidation has commenced immediately around the tube. The bronchial tubes in the consolidated tissue are frequently dilated and thickened, and are seen to stand out prominently above the level of the cut surface, surrounded by a black dense case, consisting of the indurated and deeply pigmented tissue immediately around the tubes. When the tube is of small size, it is only on close examination that its orifice can be discovered in the centre of the black projection. Even those parts of the lungs which are still crepitant are for the most part denser and less elastic than normal lungs, and they are also very frequently traversed by fibrous bands or tracts similar to those seen in the nodules. In some specimens these tracts are narrow and not very visible to the naked eye, but in others they are broad and very obvious. When cut into, the lungs usually exude more or less of a black fluid, which closely resembles Indian ink in appearance. This fluid is most abundant in the lungs of miners, from the crepitant portions of which it often pours out in large quantities. When evaporated to dryness, it leaves a black residue which is quite insoluble by boiling in strong nitro-hydrochloric acid."

Do other morbid processes occur of less obvious origin giving rise to the familiar characters of pulmonary consumption?

Dr. Charles Theodore Williams[a] contributes some important facts relative to the causation of consumption, mainly drawn from the experience of Dr. C. J. B. Williams. Out of 800 cases

[a] "The Causes of Pulmonary Consumption," by C. T. Williams, M.D.—St. George's Hospital Reports, 1869, p. 153.

tabulated from the private practice of the latter gentleman, on the ground that each patient had been at least one year under observation, it was found that 101 or about one-eighth originated in bronchitis and influenza; of these 47 were free from hereditary predisposition. Out of the same number 121 cases could be traced to attacks of pneumonia and pleurisy, and in 69 of these there was no evidence of hereditary predisposition. It is probable that an even larger proportion than this of cases of consumption originating from local causes would be found among the poorer classes, inasmuch as these figures have been drawn from the private practice of an eminent London physician.

Perhaps the most distinguished advocate of the doctrine, that consumption in many cases owes its origin to other than tubercular causes is Professor Niemeyer[a] of Tubingen. It is mainly to the writings of this eminent physician that the spread of this opinion is owing. Niemeyer has collected a large amount of both pathological and clinical evidence to show that consumption is often produced by chronic inflammatory processes in the lungs, the affection which, in his opinion, most frequently engenders it being chronic catarrhal pneumonia. This disease is usually preceded and accompanied by bronchitis—from the bronchi the morbid process extends to the air-cells, so that these as well as the smallest tubes become filled with the products of the catarrhal inflammation. These are the cells and fluid usually met with in inflammations of the mucous membranes, and as the fluid becomes absorbed, the solid elements undergo the caseous or cheesy degeneration—a process which plays a very important part in the theory of consumption, and which ultimately leads to softening of the pulmonary tissue.

Other cases of consumption take their origin according to the same observer in hemoptysis. We have been in the habit of believing that pulmonary hemorrhage is in the majority of cases an evidence of the existence of tubercle, and not a cause of phthisis. According to Niemeyer, of those who have hemoptysis a considerable number are not phthisical, and never become so. The effused blood in some instances excites the inflammatory process, giving rise to lobular pneumonia, the portion of extravasated blood remaining in the lung sharing in the same process of degeneration as that which the inflammatory products undergo. In cases in which this

[a] Berlin Klin. Woch. Nos. 17 & 18, 1869. Text-book of Practical Medicine. Trans. by Humphreys and Hackley. New York, 1869.

effect is produced by hemorrhage, Niemeyer has found that, in about three days after the attack, febrile symptoms manifest themselves, the temperature rises, and pain is felt in some part of the chest. In this part fine râles become audible, the respiratory sounds are altered, and pleural friction is occasionally manifest, along with these is dulness on percussion.

In some instances hemoptysis is secondary to an inflammatory process already existing in the lungs, a sequence which can be recognized by observing that the febrile condition, and the other evidences of inflammation accompany or precede the appearance of blood in the expectoration. Another source of danger from hemoptysis consists in the absorption of portions of the degenerated blood, and consequent infection of the system.[a]

If we turn now to the pathological evidence and inquire what grounds there are for supposing that the destructive changes in the lungs met with in *post mortem* examinations of persons who have died of pulmonary phthisis are always of a tuberculous nature, we find that the great point to be decided is, what is the nature of the yellow cheesy substance found in the lung, and which Laennec designated as crude yellow tubercle. Without entering into unnecessary details we may say that the progress of microscopical observation enables us to state with certainty, that this substance is in many instances not tubercle at all. A material altogether undistinguishable from this so-called tubercle is produced by the drying-up of pus, by the degeneration of lymphoma or syphilitic deposits, and is even found in cancerous growths as well as under various other circumstances. In some cases this material is undoubtedly tubercle, and the classical description of Laennec as to the change produced in the gray granulations has been too often verified to be seriously disputed. But the only unquestionable evidence of the existence of tubercle consists in the presence of the gray granulation, and not in that of the yellow substance which may be tubercle, but which may also be one of a number of other things; nor in the existence of cavities, which, as we have seen, may result from very different causes.

Even when gray granulations are present, Niemeyer holds that they are in many cases secondary to other pathological changes.

[a] In a very interesting memoir which has been translated by Dr. W. D. Moore, Dr. Rasmussen, of Copenhagen, has detailed a number of cases of hemoptysis in which aneurisms of small branches of the pulmonary artery existed, which had by their rupture given rise to the hemoptysis. This condition has now been found by the author in eleven cases.—Edinburgh Medical Journal, August, 1869.

We shall have occasion to explain, in connexion with the origin of tubercle, how this secondary tubercular formation is supposed to be brought about.

When the now celebrated experiments of Villemin, of the results of which an account was given in this Journal in August, 1868, were published, the possibility of the artificial production of tubercle by inoculation, was soon definitively established by the labours of a large number of experimentalists. That tubercle can be generated in an animal by the inoculation of tubercle can now be no longer called in question, consequently infective powers must be conceded to exist in it. But there seems equally little doubt that tubercle may be generated by the introduction into the system of other substances, even of inorganic bodies as well as by the irritation of setons and the like. Of this fact we have the concurrent testimony of many observers. The experiments of Cohnheim and Fränkel among others, the results of which were noticed in this Journal for May, 1869, point clearly to the origin of tubercle from morbid products which have been generated within the body. These observers found that the introduction into animals of substances, such as caoutchouc, was followed by the development of tubercle in distant organs, the necessary intermediate step being that inflammation and abscess should be excited by the substances so introduced. In instances of this kind it is clear that the organism must have become infected by morbid materials generated within itself, the altered pus being the only source to which we can refer for our explanation of the subsequent contamination. This is the process of self-infection, and it possesses great interest for the physician, inasmuch as it points to a ready explanation of various cases of consumption occurring in the human subject. It is reasonable to suppose that in man as in animals a development of miliary tubercle may be the result of the absorption of some of the products of inflammation of certain parts, when these products have reached a particular stage of degeneration. This is of course merely a hypothesis, and one which is hardly susceptible of positive proof, but it has many analogies in its favour, and it affords an intelligible explanation of serious pathological difficulties. At the same time it must be borne in mind, that when tubercle itself is employed in inoculating animals, it acts with much greater certainty in the production of tubercle than any other species of irritation or inoculation. So distinct is this difference that it establishes a probability of the specific and virulent character of

tubercle, and has led Villemin and others to the adoption of the view popularly entertained both in this country and abroad, of the contagious character of tuberculosis.

This leads us to the great practical question of how consumption is likely to be engendered in man. If we accept Niemeyer's doctrine, we must admit that bronchial catarrh and catarrhal pneumonia, are in many instances the causes to which it owes its origin. We can also admit that direct mechanical irritation of the bronchi may be the starting-point of processes which ultimately lead to consumption. We may incidentally allude to the effect of changes in gummatous syphilitic products in the same direction. All these are susceptible of direct proof, and resemble well-known processes in the organism.

Even when we come to elements of causation, for which we have to seek for proof in experiments on animals, the evidence, although weaker, is still enough to establish a good deal of probability in favour of certain modes of production.

We can readily understand that close and long continued relations with individuals suffering from phthisis, may have a direct effect in generating the disease. It is easy to comprehend that the air surrounding phthisical patients, may become at times the vehicle for transmitting to the lungs of those who are in their neighbourhood, some particles which have been brought up by the attacks of cough to which the sufferers are subject, and that in this way a direct infection analogous to an inoculation may be produced. It is of course out of the question that direct inoculation should be practised, but it is possible that what is called self-infection may be not unfrequently brought about. With regard to this clinical observation comes to the aid of experiment, and points out that in a large number of cases of consumption, some focus of infection can be ascertained to exist, for example, enlarged and degenerated lymphatic glands, purulent exudations in the pleura or in the peritoneum which have been dried up, and other similar conditions in different organs in which cheesy materials have been formed, and are retained in the system as the result of pathological processes. The general fact being that infection of the system is likely to be produced by absorption of material from any part where chronic inflammatory changes have gone on, and that this absorbed material is likely to produce a morbid process in the organs to which it has been carried. It is worth noting that the diseased organ from which the infecting

material is derived may be the lung itself, and that very frequently a chronic pneumonia may become the starting point of the infective change. This is the explanation of the apparent paradox of Niemeyer, that the greatest danger to which a consumptive patient is exposed is that he may become tubercular.

The infecting material is probably molecular, and is certainly in a very fine state of subdivision, and it is carried by the lymphatics, and by the veins so as to reach the tissues. It is worth mentioning that recently Lebert has drawn attention to a curious connexion existing between congenital stenosis of the orifice of the pulmonary artery, and tubercular disease of the lung, a fact which is of interest when we remember the comparative immunity from tubercle enjoyed by sufferers from those heart diseases which lead to congestion of the lungs.

It is impossible to explain how it occurs that the effect of this impregnation of the system with the products of its own diseased action should give rise to tubercle. It is also by no means easy to understand how it is that particular portions of the lungs should be so commonly selected for the development of morbid changes. The clinical fact of the preference of phthisis for the superior parts of the lungs would rather seem to be the effect of a species of elective affinity similar to what exists between certain morbid poisons and certain parts of the body, than of any common inflammatory process. Till some satisfactory explanation is given of this, it must be regarded as presenting a great difficulty in the way of the reception of the doctrines which we have endeavoured to explain.

A portion of the difficulty has been met in a somewhat summary fashion by statements which are certainly very startling, and of a nature which it would require much and strong evidence to induce pathologists to adopt. So far we have adopted the usual pathological view that in tubercle we have a new formation, having certain pathological relationships, but distinctive in itself. But in a paper in the *Edinburgh Med. Journal*, we find Dr. Burdon Sanderson asserting that there is nothing which is, properly speaking, specific in tubercle at all. Dr. Sanderson states that the parts which are most frequently the seat of what is at present regarded as tubercle, are those in which a particular kind of tissue is to be met with. This tissue is identical with that of the follicles of the

[a] Medical Times and Gazette, Dec., 1869.

lymphatic glands, and he proposed to designate it as adenoid tissue. According to this distinguished observer, tubercle in serous membranes is essentially an overgrowth of gland-like or adenoid structures, which are normally found in these membranes. In the lungs the process is somewhat more complex. A substance identical with this adenoid or gland-like tissue is produced in the alveolar septa, and at the same time cells multiply to a large extent in the alveolar cavities. The result of these changes is that small masses having the characters of lobular pneumonia are produced. These masses by their increase gradually coalesce with each other, they undergo in their centres the well-known cheesy or caseous degeneration, become softened and ultimately a cavity is formed. In all this, according to Dr. Sanderson, there is nothing structurally either specific or malignant, and he suggests that it would be productive of great benefit to pathological science, to get rid of the word tuberculous altogether, inasmuch as it has, in reality, no anatomical or chemical basis. Dr. Sanderson's conclusion is as follows:—

"Anatomically the cardinal fact is this: All cases of phthisis pulmonalis have this in common, that they commence with lobular induration, and end in disintegration and cavern-formation. In no generally understood sense can a case of acute miliary tuberculosis be said to be a case of phthisis. This broad definition of phthisis brings us back once more to Laennec. It was he indeed who first recognized the fixed relation between induration and disintegration as antecedent and consequent; but he unfortunately extended the notion from the tissue to his 'tuberculous material.' The conclusion to which we have now arrived by anatomical investigation is this: There is no difference structurally between the induration of phthisis and the induration of chronic pneumonia. There are no anatomical elements in the hard parts of a phthisical lung which are not to be found in the hard parts of chronically irritated tissue in any other organ. Finally, there are no elements in the induration of phthisis which are not to be found in chronic bronchitis. It is even possible that there are no anatomical elements in the induration of phthisis which are not to be found in the healthy lung. We cannot say this positively as regards man, but it is certainly so as regards the guinea-pig." [a]

Should further observations confirm the opinions which we have thus briefly sketched, the modifications which would necessarily follow in our management of consumptive patients, would neither be few nor inconsiderable. We should recommend that the bed of a

[a] Edinburgh Med. Jour., Novr. 1869, p. 393.

phthisical person should not be shared by any other individual, we should regard hemoptysis with less apprehension as an evidence of phthisis, but with far more apprehension as a cause of phthisis, and accordingly we should direct our efforts to promote the expectoration of the effused blood, we should look upon a common bronchial catarrh, arising in a delicate person as an ailment which may be followed by very serious consequences, we should be much more solicitous than we at present are, to guard the lung from the irritating effects of exposure or of changes of temperature. We should endeavour to hinder the development of the bronchial catarrh which may be followed by so formidable results, and we should be specially solicitous about the prevention of the supervention of lobular pneumonia on these apparently trifling ailments. Even in the more advanced stages of the disease, we should be less impressed with the conviction of the fatality of consumption than we now are. At present our belief in the essentially constitutional nature of consumption leads to a treatment based on this idea, to a strict attention to the general condition, and to a comparative neglect of the merely local manifestations. The hopeless view of the disease expressed by Laennec, when he said that "almost all professional men are now of the opinion that the tuberculous affection is, like the cancerous, absolutely incurable, because nature makes no efforts, but what are opposed to cure, and art can only make useless ones;" has long been regarded by physicians as too absolute. The result of recent researches will be to encourage the adoption of a still more hopeful view, and to lead to renewed efforts in the direction of the therapeutics of the malady. Certainly enough has been shown to render it in every way desirable that further investigations should be made to endeavour to arrive at definite conclusions regarding the nature of this terrible malady, and to establish its treatment on a solid and rational basis.

TREATMENT OF ANEURISM.

Dr. George W. Balfour[a] contributes a new series of cases of internal aneurism, treated by iodide of potassium, in doses of 20 or 30 grains, three times daily. His increased experience has, he believes, fully justified the opinion formerly expressed by him regarding the efficacy of this mode of treatment, as being the best hitherto devised for the relief, if not cure, of this formidable malady.

[a] Edinburgh Medical Journal, 1869.

Dr. Von Langenbeck[a] has employed the subcutaneous injection of ergotine, in the treatment of aneurism, in two cases. In one, an aneurism of the radial artery, a single injection was sufficient to cure the disease: in the other a subclavian aneurism, the remedy was used with much benefit. He believes the ergotine to act, by exciting contraction of the involuntary muscular fibres in the arterial wall.

Professor Zdekaner[b] speaks highly of the treatment of aneurism of the aorta, by the galvanic method. He introduces three to six steel needles at points about an inch and a quarter distant from each other. They are to be inserted in a convergent direction; but so that the points do not to touch. He employs moderate currents, and connects the needles with the positive pole of the battery; the negative pole being connected with a sponge, and applied at different parts of the tumour.

Hoegh[c] relates a case in which an aneurism of the arch of the aorta, was cured by the administration of large doses of acetate of lead. Four grains were given morning and evening, and the dose was increased by two grains every eighth day. When the dose reached 30 grains, symptoms of lead poisoning appeared. which yielded to treatment within a month. By this time the pulsation in the tumour had diminished. Some months after, the tumour having again begun to increase, the lead treatment was again resorted to with benefit. Five separate courses of the acetate were given during four years; the administration of the remedy being pushed to the point of the production of symptoms of lead poisoning, with the ultimate result of cure of the disease.

TREATMENT OF DIABETES MELLITUS.

In 1865 Saikowsky[d] found that the administration of arsenic to animals, had the effect of rendering the liver free from the sugar-forming or glyco-genic substance. The effect of the administration of arsenic in two cases of diabetes, has been recorded by Dr. Leube[d] the notion of the possible utility of this drug, in such cases, having been suggested to him by these experiments. The results were satisfactory in both cases; the amount of sugar in the urine diminishing notably, without any change in the diet of the patient. The first indication of improvement was a diminution in the amount of the urine, not in the proportion of sugar contained it. The remedy was

[a] Berlin Klin. Woch., 1869. No. 12.

[b] St. Petersburgh Med. Zeitschrift, 1870. 1 Hft. Prager Viertel-Jahrschrift, 1870, s. 23.

[c] Norsk Magaz. for Laegevidensk, Bd. 22, Hft. 6. Virchows Jahresbericht, 1869.

[d] Deutches Archiv fur Klinische Medicin. Band 5, S 572.

prescribed in the form of Fowler's solution; and in one case $\frac{1}{3}$ grain of arsenic was taken daily for two months without inconvenience.

Dr. Owen Rees[a] had, in 1864, employed a solution of chloride of arsenic, with excellent effect in a case of diabetes, in which other remedies had failed.

Dr. Jaccoud[b] has found, that arsenic with a suitable dietary, has been sufficient to cure several cases of diabetes, in which the patients had not as yet lost flesh. He has not tried it in cases, in which much emaciation existed. He has also found the administration of strychnia, to be followed by excellent results. He uses the sulphate of strychnia dissolved in water, with the addition of syrup of orange peel; the dose at the beginning being $\frac{1}{13}$ of a grain, which is to be increased to four or five times this amount.

In a very interesting series of communications, Dr. Arthur Scott Donkin,[c] Lecturer on Forensic Medicine to the University of Durham; advocates the employment of purely milk diet in diabetes mellitus, Brights disease, disease of the supra renal capsules, and other diseases accompanied by imperfect nutrition. He records two cases of diabetes, in which the milk diet was methodically persevered in. In the one, the patient was rapidly restored to a condition of health and strength; only a little sugar remaining in the urine, to show that the disease was not completely eradicated. In the other however, the cure was rapid and complete. In the former case four grain doses of opium were given at bed-time; and one grain of sulphate of iron, and three grains of sulphate of quinine, in mixture, three times daily. In the latter, one grain of sulphate of iron, and two grains of sulphate of quinine, were given three times daily, This with six pints of skimmed milk, warmed, and divided into four meals, with an interval of four hours between each, constituted the entire dietary and treatment of the case from the beginning.

With regard to the rationale of the treatment, Dr. Donkin observes as follows:—

"Dr. Francis Home first indicated the value of an animal diet in the treatment of diabetes mellitus, and it is now nearly three-quarters of a century since the diet he indicated was reduced to a system by Dr. Rollo, in the important work he published on the subject. During the period which has since elapsed a diet more or less purely animal, as indicated

[a] Lancet: October, 1864.

[b] Nouveau Dictionnaire de Medecine et de Chirurgie. Tome 11, 1860.

[c] Lancet, November 27, 1869, p. 736.

by him, has been recognized as the proper and only available treatment to be employed against this most formidable malady. But the treatment of Rollo, either absolutely followed out or modified by the admission of certain kinds of vegetable food, is not by any means so universally successful as could be wished. The life of the patient is sometimes cut short during the treatment by the sudden accession of inflammatory affections of the abdominal and thoracic viscera. Dr. Bardsley specially directed attention to this fact. Besides, it is exceedingly difficult to enforce a strict adherence to animal food for any length of time; with some individuals, indeed, it becomes almost intolerable. Dr. Rollo himself even lamented that this mode of treatment should be so repulsive to the patient. A purely milk diet, however, is free from these objections; milk being generally relished by the patients, especially at the outset of the treatment, when the thirst is intense. Afterwards they never tire of it, although they generally express a desire for solid food in addition.

"From the experience I have already had, I have not the slightest hesitation in asserting that the milk treatment of diabetes is as superior to Rollo's, with its more modern modifications, as Rollo's was to any method previously in use. But it is necessary to add that *it must be persevered in methodically and exclusively until convalescence is established.* If this rule is not followed from the beginning the result will not be successful.

"The efficacy of this treatment need excite no surprise when we know that the disease has hitherto been amenable only to the protracted employment of a dietetic course. The *rapidity* with which milk acts is truly surprising, twenty-four hours being sufficient for the production of a marked improvement: the quantity and density of the urine suddenly fall, and, *pari passu* with this change, the thirst and voracious appetite disappear, the skin becomes moist and perspiring, while the symptoms referable to the nervous system are as rapidly relieved; profound refreshing sleep succeeds to the previously sleepless, restless condition, rendered almost intolerable by the incessant thirst. The patient expresses himself thankful for the sudden and unexpected change. In the two cases I have recorded this sudden relief was obtained. I may mention, too, that a third case is now under my care, in which by the same milk diet, and *without any other remedy whatever*, the urine fell from 23 pints, sp. gr. 1038, to 9 pints, sp. gr. 1040, in twenty-four hours, and to 6 pints, sp. gr. 1038, at the end of the third day of treatment; in other words, there was *a diminution of* 14 *pints of urine and about* 19 *oz. of sugar in twenty-four hours, and of* 17 *pints and about* 24 *oz. of sugar in three days.* A result like this seems almost incredible; but I may add that this astonishingly rapid improvement in the urine was accompanied by a corresponding rapid abatement of the other prominent symptoms of the disease. I shall, however, give a complete history of this in a subsequent communication.

I have no doubt that others will, on trial, find the remedy to act with the same magical rapidity as I have done."

Dr. George W. Balfour[a] also has found a dietary consisting exclusively of milk, as employed by Dr. Donkin, of great service in a case of diabetes. The patient, a woman, was put on six pints of milk for her sole diet, no other food whatever being taken, with the effect of reducing the amount of urine one-half, and diminishing, also, the specific gravity. This mode of treatment being simple and inexpensive, can, as he suggests, be readily carried out at home, and need not exclude the employment of such drugs as may be considered advisable.

Dr. Pavy[b] states that of all the agents which he has hitherto employed in the treatment of diabetes, opium, morphia, and codeine, have the most powerful influence in controlling the disease. Of the three remedies named, he believes codeine to have the best effect. In some cases, it has exercised a directly curative action; in others, it has mitigated the severity of the malady. The following observations on codeine, will be found to possess interest:—

"Of opium, morphia, and codeine, the last, I consider, possesses the most advantageous properties. As far as my experience, including that amongst hospital in-patients and out-patients and in private practice, enables me to judge, I cannot help thinking that codeine will be found to prove a valuable agent in the treatment of diabetes. In some cases it appears to possess the power of exerting a direct curative action. In others, however, although it has been found, particularly for a while, to control the elimination of sugar and improve the general condition, yet it must unhappily be said that it has afterwards proved powerless in averting an unfavourable termination.

"The great advantage of codeine over opium and morphia is that it is equally efficacious in controlling the disease, and does not exert the same narcotic effect. When given in a small dose to begin with, and increased gradually, nothing may be perceived beyond its effect upon the disease. Half a grain I consider an appropriate dose to begin with, and, as seen by the following cases, I have given it to the extent of ten grains three times daily. Beginning with one grain three times a day, a little drowsiness, giddiness, and perhaps headache, may be complained of. I have begun with two grains three times a day; but the dose had to be reduced to one grain on account of the disturbance of the cerebral functions that was produced. Under its employment I have not observed the pupils to

[a] Edinburgh Medical Journal, February, 1870.
[b] Guy's Hospital Reports, 1870.

become contracted, nor have I noticed any marked constipating action. It may, perhaps, have a little tendency this way, but nothing certainly to any marked extent. The tongue, in some of the cases, I have particularly noticed to be conspicuously clean and moist. The appetite, as a rule, has not been affected by it."

ON SYNCOPE.

Professor Brown-Sequard[a] announces that three varieties of syncope exist. Of these, one is familiarly known, namely that caused by cessation of the hearts action.

The second variety is less known, it consists in the stoppage of the respiratory movements, without asphyxia When the respiratory movements stop suddenly in consequence of irritation of the medulla, or of the neighbouring parts of the nervous centres; the heart continues, in the majority of cases, to beat with more or less rapidity and force. The stoppage of the respiration is not due to spasm of the diaphragm, or of the intercostal muscles, but to a cessation of action in them, brought about by a cessation of action in the respiratory nervous centre.

The third variety of syncope has not been before observed, and the author merely directs attention to it, promising to explain it more fully at a future period. In making experiments on the cause of the absence of agony in sudden death produced by irritation of the vagus, of the sympathetic nerve in the abdomen, of the spinal cord, or of the encephalon, Dr. Brown-Sequard had ascertained that the venous blood was less deep in colour than usual, and that it was sometimes as red as the arterial. Besides this, he observed a rapid lowering of temperature; and from these two facts, combined, he has come to the conclusion that certain of the interchanges which normally take place between the blood and the tissues, are either much diminished or altogether arrested.

It is remarkable that in animals in whom these phenomena have been produced, the vital properties of the nervous and contractile tissues persist much longer than when death has been caused by asphyxia, or by sudden hemorrhages.

GLOSSO-LARYNGEAL PARALYSIS.

Dr. Wilks[b] gives an account of four cases of this affection, in one of which, a post mortem examination was made by Dr. Moxon.

[a] Archives de Physiologie Novembre et Decembre, 1869.

[b] Guy's Hospital Reports, 1870, p. 1.

In this case, which was complicated by the co-existence of progressive muscular atrophy, the characteristic symptoms of this form of paralysis were present. The orbicularis oris was perfectly paralyzed, so that the saliva escaped from the mouth; the buccinators were paralyzed: the patient had lost power over the tongue, and his speech was scarcely intelligible. There were other symptoms present referable to the progressive muscular atrophy. The post mortem showed atrophy of the roots of the hypoglossal, of the inner roots of the spinal accessory, and of the whole of the roots of the anterior spinal nerves. The anterior half of the spinal cord showed atrophy, and hardness of the white substance, the grey substance being enlarged and reddened.

There was a red-grey change visible in the floor of the fourth ventricle, involving the nib of the calamus scriptorius, and extending upwards and outwards for half an inch.

Dr. Wilks draws attention to the dissections of Mr. Lockhart Clarke, which demonstrate the connexion between nuclei of the facial, vagus, hypoglossal, and laryngeal nerves, and offers the following ingenious suggestions:—

"It appears remarkable that a small area in the medulla oblongata, or even the olivary body, coinciding with a physiological centre as that of articulation, should be picked out to undergo a rapid or slow morbid change. So remarkable is the fact that it might be worthy, in the first place, of inquiry whether or not experience justifies us in declaring that definite parts of the cerebro-spinal centres having special functions are more prone to disease than other portions of the brain and spinal cord taken indifferently; whether, indeed, all parts are not equally liable to inflammation and degeneration, but it is only when certain physiological portions are affected that we are enabled to apply our names, because then the seat of disease has made itself manifest by the implication of nerves whose function is known, whilst in other cases we are content to use such expressions as cerebral or spinal disease. Although I believe this to be to a certain extent true, yet I consider it is proved that those parts of the cerebro-spinal system which have definite physiological properties are more liable to disease than other spots taken indiscriminately; if so it may show, as is most probably the case, that the vascular supply of such physiological centres is accurately defined and circumscribed, or it may be that a centre having a definite function, being the focus of a number of nerve filaments proceeding from it for a special purpose, must soon be affected if any of these filaments be primarily attacked, seeing that morbid processes quickly proceed along given anatomical tracks. If, then, degenerative processes occur in connexion with a morbid state of blood-

vessels, and if the anatomical supply bears a relation to defined physiological areas, the explanation of such parts being selected for chronic disease is not so difficult; and if again morbid processes proceed rapidly along nerve filaments, we can understand also how parts having intimate relations are concurrently affected. It is remarkable circumstance that there is no disease of the nervous system, as far as I am aware, which may prove fatal, and even show a well-marked lesion or degenerative change after death, but may have its counterpart in a functional and curable disorder. This must clearly show that, although in actual blood effusions or softenings the seat of disease may appear accidental, these centres must have been affected from a more precise anatomical reason. For example, an effusion of blood in the corpus striatum productive of a hemiplegia may appear to be accidental, but a temporary hemiplegia of a few hours' duration, as seen after an epileptic fit, and due to a derangement of a part having a very limited area, can only be accounted for on the supposition that physiological districts of the brain are anatomically defined. It is the same with diseases of the spinal cord, every variety of which may occur as a temporary disorder. Let it be said, for example, that paralysis of motion and sensation imply definite lesions of the motor and sensory tracts, then the function of these tracts must be in temporary abeyance to account for the paralysis which occurs in those patients who perfectly recover. Again, let it be said that distinct nervous lesions are discovered in cases of progressive muscular atrophy; we must suppose that in those very severe cases which recover these same special parts of the cord were for a time powerless. Or, if in locomotor ataxy it be shown that the posterior columns of the cord have undergone grey degeneration, it must be believed that those same parts are involved in some way in the cases which recover. All these circumstances would show that of the various nervous ailments which we meet with a large number have a determinate and fixed character owing to certain distinct anatomical and physiological centres being involved. In hysteria it is known that every possible nervous disorder may be simulated, and amongst these I have seen a tolerably fair example of labio-glosso-laryngeal paralysis."

ACTION OF DIGITALIS ON THE HEART.

Dr. Paul Lorain[a] furnishes an interesting contribution to our knowledge of the effect of digitalis on the heart, which he has studied by aid of the sphygmograph.

The conclusions at which he has arrived are—

[a] Journal de L'Anatomie, &c., Mars et Avril, 1870.

1st.—That in moderate doses, digitalis restores the force of the heart, and renders its action slow and regular.

2nd.—That administered in excessive quantity, or for too long a time, it deranges the action of the heart, and causes irregularity of the pulse.

In the case of a man of seventy years, suffering from mitral incompetence and anasarca, under the influence of digitalis in powder, to the amount of $2\frac{1}{2}$ grains daily, the pulse which had been quick, weak, and irregular, became stronger, more frequent, and perfectly regular. When the remedy was continued too long, or the dose unduly increased, the pulse became very slow, falling to 32 in the minute. Dr. Lorain has observed that in cases in which the pulse has become slow under the influence of this drug, the cardiac contractions are more numerous than the beats of the pulse which are to be felt in the wrist. When the pulse was 32, the stethoscope enabled him to recognize that there were 64 systoles of the heart, that is exactly two ventricular contractions for each beat of the pulse. Of these, one, that which corresponded to the pulse was strong; the other was very weak, but distinctly audible; and its effect, although too slight to be perceptible by the finger, was to be recognized in the sphygmographic tracing.

PUNCTURE OF THE ABDOMEN IN TYMPANITES.

Dr. Julius Stein[a] details two cases in which the abdomen was punctured with a fine trocar for the relief of gaseous distension, with excellent effect.

In one patient in whom enormous distension of the abdomen was produced by the pressure of an ovarian tumour on the intestine; the puncture was made in the cecal region, and was repeated daily, at the urgent request of the patient; over fifty punctures having been made during the last period of the disease. At the post mortem hardly a trace of these numerous punctures could be observed, and there was absolutely no evidence of any irritation of the intestine having been produced by them.

In the second case, that of a man of sixty-one years, eight successive punctures were made during fourteen days. The trocar was inserted, in this case, below the ensiform cartillage, and, as in

[a] Deutches Archiv. fur Klinische, Medicin. Band, VI., heft 4. 1869.

the previous case, the operation gave great relief to the patient; and was followed by no unpleasant sequelæ.

Dr. T. Clifford Allbutt[a] details the following interesting case:—

"——, aged 40, labourer, was admitted into the Leeds Infirmary on January the 4th. He was admitted as suffering from 'obstruction of the bowels.' On investigation, Mr. Bradley found that although the bowel symptoms were more prominent, yet the patient was suffering also from double pneumonia. He told me that he was led to examine the lungs because he had seen obstruction of the bowels more than once in connexion with pneumonia. I was also aware of this co-existence of paralysed bowel with pneumonia, and have explained it by supposing that irritation from the inflamed structures reaches, and finally exhausts, the colon. I have certainly seen it in several well-marked cases, when it has proved a very distressing and unmanageable symptom. My explanation seemed certainly to be true in a somewhat similar case of bowel paralysis attended by Sir William Jenner, Dr. Beaumont of Knaresborough, Mr. Bainbridge of Harrogate, and myself,[b] where paralysis of the bowel was set up by inflammation near it, and such paralysis, we know, often occurs in peritonitis. In such cases of simple paralysis, or of fecal accumulation with distension, I think the mode of treatment I am about to propose may turn out to be of great importance. In the present case Mr. Bradley administered subcutaneous injections of morphia, and full enemata both with and without turpentine and castor-oil. No relief was obtained. No feces had now passsed from the bowel for at least five days, and no wind had passed for about two days. The abdomen was enormously distended, and caused intense suffering; the pulse was small and quick, the extremities cold, and the countenance very anxious. In the evening Mr. Bradley requested me to see the patient. His state remained the same. I advised an injection of a quart or more of warm gruel to be slowly administered with a long tube, this to be followed by an injection of half a pint of warm olive-oil in the hope that the oil might rise through the gruel and reach the obstruction. Belladonna linament was also to be applied all over the abdomen, and the subcutaneous injections to be continued. About three pints of the fluid were got into the bowel, but no feculent discharge followed: the linament gave some relief. Next morning I found the patient in great suffering; he had not passed any wind, and he had spent a wretched night. He now had also a decided hiccough, and the abdomen was enormously distended. There neither was, nor had been, much vomiting.

[a] The Practitioner, February, 1869, p. 109.
[b] Vide Lancet, July 18, 1868.

The previous evening I had been much tempted to puncture the abdomen with a fine trochar, and in the morning the desire strongly returned. Somewhat later in the day I was with Mr. Teale, and after laying the case before him, I asked him if he would operate for me. He agreed to do so, and we visited the infirmary at once. We found the patient in great distress; he was tossing restlessly about in bed, complaining of the distension of his abdomen, and the breathing was seriously impeded. Mr. Teale plunged an exploring trochar (No. 1. Weiss) into the distended transverse colon. On withdrawing the trochar air rushed through the canula with a hissing noise and the abdomen slowly fell. The air was highly offensive. When the current grew weaker Mr. Teale made a second puncture into the descending colon, which was greatly swollen: the result was the same. The body had now fallen much, and had become quite soft; the patient expressed the greatest relief, and thanked us warmly. His countenance became placid, the breathing comparatively easy, and he soon sank into sleep. I ordered the morphia to be continued, and poultices to be kept on the body. On my visit next day I found our treatment had been remarkably successful. Not only had the relief continued, and the poor man slept comfortably, but the bowels had recovered their activity. He had passed a great quantity of wind to his great comfort, and many stools; some of these stools were seen by Mr. Bradley, who reports that they were copious and feculent. So far, then the operation seeemed to have been eminently successful. Unfortunately the pneumonia had not stayed its course, and the whole of one lung behind was now consolidated, and more than half of the other. He sank from the lung mischief in about two days more—the abdomen becoming again somewhat distended before death. At the autopsy, almost the whole of the left lung was found in a state of 'hepatization, and a large part of the right lung also. The other organs were healthy; in particular there was no trace of peritonitis, nor had any air escaped into the peritoneal cavity. No traces of the punctures could be detected except upon the outside of the body. I need not stay to point out how often patients beg for relief from a distended abdomen, nor, I think, to show that the simple and painless operation which we practised proved to be quite without danger, and gave great and immediate relief, not only by emptying the bowel of wind, but by enabling it to contract upon, and to expel, the feces which obstructed it. The operation is one which any physician may have recourse to in a moment, and if he has not a fine trochar at hand he may use the needle of his morphia syringe. I may add, that the autopsy did not seem to support my view of the causation of the bowel paralysis. There was no appearance of inflammation on the under side of the diaphragm, nor was the surface of the bowel injected with blood.'"

DIAGNOSIS OF RENAL DISEASE FROM BLOOD-CORPUSCLES IN THE URINE.

Dr. Joseph G. Richardson,[a] Microscopist, to the Pennsylvania Hospital, Philadelphia, has applied to the elucidation of the condition of the kidney in renal disease, the well-known views of Cohnheim, regarding the relation of the white blood-corpuscles to the inflammatory process. If both red and white corpuscles are present in the urine, and if it can be ascertained that their occurrence is independent of vesical irritation, the following indication may be derived from their relative proportion.

When the red corpuscles greatly exceed the white in number, true hemorrhage has taken place from the kidney. This conclusion will be strengthened if no tube-casts be found, and the albumen be present in such quantity as will correspond to the amount of corpuscles, supposing both to be furnished by the liquor sanguinis.

When the white corpuscles are more than $\frac{1}{2\,3}$th of the red, there is inflammation of the kidney of either an acute or a sub-acute kind, and the danger will be found to be in direct proportion to the total amount of the corpuscles.

When few or no red corpuscles are found to be mixed with the white chronic inflammation is present, and is proportionate in extent to the number of blood-corpuscles.

In cases of acute Bright's disease, passing into the stage of fatty degeneration, the appearance in the urine of epithelium, containing oil globules points to the change from the stage of inflammation, and these epithelial cells will be found, according to the author, to vary inversely with the number of white corpuscles present, the albumen remaining the same in amount.

[a] American Journal of the Medical Sciences, January, 1870.

REPORT ON OBSTETRIC MEDICINE AND SURGERY.[a]

By George H. Kidd, F.R.C.S.I., L.K. & Q.C.P.; Obstetric Surgeon to the Coombe Lying-in Hospital; Vice-Pres. Obstet. Soc., Ireland; Cor. Mem. of the Gynecological Soc. of Boston.

STATE OF THE PULSE IMMEDIATELY BEFORE AND IMMEDIATELY AFTER PARTURITION.

The pulse during the puerperal state was made the subject of a series of observations by Dr. Hémey[b] in 1864, while serving as interne under Dr. Empis, and he believes his observations established two remarkable phenomena:—First, the lowering of the pulse (already noted and studied by M. Blot), with regard to which Dr. Hémey says his observations confirm those of Blot. Second, the inequality and irregularity of the pulse, which had not hitherto attracted attention.

Dr. Hémey's observations seem to have been made with very great care, and are recorded with great minuteness of detail, and therefore seem worthy of having an abstract of them recorded here; but the results arrived at are different from those of Dr. M'Clintock, published in this Journal in May, 1861; and also from those arrived at from a series of observations made by the writer of this report in the Coombe Lying-in Hospital.

Dr. Hémey takes 75 as the average pulse of a healthy woman. Before labour it is from 75 to 84; but he does not count any pulse as lowered, unless it is 60 or under, and by adopting this as his standard, he believes he excludes all doubtful cases from his tables. He thinks at least one-sixth of all newly confined women have their pulse lowered. In one case the pulse was as low as 44; in many he found it 48; and in a great number 52. The lowering shows itself most frequently some hours after labour, and continues sometimes without variation; in some it increases during the eight days. At the end of the third, fourth, or fifth day the pulse sometimes becomes relatively more frequent and falls again, or it may preserve this frequency to the end; but these changes seem to depend on accidental circumstances, and independently of such the lowering appears to have its maximum at the end of the second day from labour, and to diminish gradually and cease about the tenth day.

[a] The author of this Report, anxious that every contribution to obstetric medicine and surgery should be noticed, will be glad to receive publications on the subject. They may be sent to the publishers of the Journal through their correspondents.

[b] Recherches sur le Pouls Pendant les Quinze Jours qui Précèdent on qui Suivent l'Accouchement. Par le Dr. L. Hémey. Archives Générales de Médecine, Vol. II p. 154.

Out of 100 consecutive cases in which the daily state of the pulse was recorded in the Coombe Lying-in Hospital, it was found as low as 60 in five cases and within the first 24 hours after delivery; and in each it had risen to from 70 to 84 on the second day, and maintained this average as long as the patient was in the hospital. In one case the pulse was as low as 56 eight hours after labour. This woman had been delivered in the street, on her way to the hospital. On the second day the pulse was 72, at which it remained till the fourth day, when it fell to 65. On the fifth day it was 72; and on the seventh, which is the last observation recorded, it was 84. This was the only case of the series in which the pulse was under 60.

The duration of labour, unless it be prolonged so as to do local or general injury, does not seem to influence the changes the pulse undergoes, but the occurrence of gastric derangement with headache, of a foul tongue with constipation and hot skin, on the second day may raise a pulse to 80 or 88, which the evening before was only 56, and Dr. Hémey observes, as has already been remarked by M. Blot, this increase of 30 is of more importance than would attach to the total number of 80, and the increase is soon proved to belong to a pathological condition by the action of a purgative which reduces the pulse again to 54 or 60. When there is metritis or sloughing of the vagina, or puerperal fever, the characteristic changes on the pulse are more permanent.

Dr. Hémey combined with his researches on the pulse some observations as to the presence of sugar in the urine of women lately confined. M. Blot announced its existence, in a paper read at the Academy in 1856, and M. Lecomte denied it in a communication read in the following June; but in 1859, Professor Bruecke confirmed the statements of M. Blot. Dr. Hémey found that of one hundred newly confined women, twenty-seven had a quantity of sugar, varying from eight to eighteen grammes per litre in their urine; the urine of thirty-five presented traces of it only; and in thirty-eight no appreciable trace of it could be discovered. Its presence was of short duration; even when most abundant, it could be found for a day or two only, and it did not seem to be in any way connected with the secretion of milk, and it did not seem to influence the condition of the pulse, for though in fifteen or twenty-seven cases where it abounded, the pulse was quick; in twelve other cases it was remarkably slow.

With regard to the influence of the secretion of milk, Dr. Hémey found that from the second to the fifth day, the pulse did not

undergo any notable change in two hundred and eighty women out of four hundred; he believes, however, that in many cases where the pulse did not seem to undergo any change from the second to the fourth day, it was because the lowering of it was interfered with by the state of the breasts, and he finds the best nurses, and those who have the most abundant supply of milk, are those in whom the pulse is lowest and that without interruption from the second to the tenth day.

The period of pregnancy at which labour occurs does not seem to influence the changes undergone by the pulse in any way. In one case when it occurred at the end of the fifth month in consequence of a fall, the pulse, which before labour was 80, fell to 76, 68, and 52, on the following days, and then rose again to 72; but in other cases of premature labour no lowering of the pulse was observed. Where the fetus is dead for some time before labour sets in, the lowering of the pulse seems to begin at the time of the child's death, rather than when it is expelled, and Dr. Hémey gives a series of cases illustrative of this as the fact is made use of in progress of the essay.

After-pains may not only, as M. Blot has already remarked, coincide with a slow pulse; but, according to Dr. Hémey's observations, when they occur independently of any other pathological condition, they are rarely accompanied by a quick pulse. Moral emotions, however, have a great influence in raising the pulse; diet does not affect it unless the process of digestion be deranged, and then the pulse is more easily affected in the puerperal state than it would be at other times. Change of position will also affect it more notably at this time, so that a woman lying down may have her pulse only 68, and it may rise to 104 when she sits up. Primiparæ seem less disposed to have the pulse lowered than multiparaæ, perhaps because the former are more subject to the multitude of little causes which tend to accelerate it.

As a prognostic sign, Dr. Hémey looks on this lowering of the pulse as most favourable. It occurs most frequently when the health of puerperal women is best, and it is oftenest met with among the vigorous and those who have the most abundant supply of milk; and he has never known a woman who presented it to be affected with puerperal symptoms of any gravity.

As to the cause of this lowering of the pulse, Dr. Hémey argues that it depends on an increased arterial tension caused by the rapid withdrawal from the general circulation of a certain number of arterial trunks; that is to say an increased arterial tension caused by

the large quantity of blood hitherto required by the uterus being thrown into the general circulation. It may be regarded as proved, he says, and it has been demonstrated by the sphygmograph, that the frequency of the pulse is in the inverse ratio to the arterial tension; and it can also be demonstrated by the same instrument, that the arterial tension is increased by compressing one or more arterial trunks, so as to withdraw them from the general circulation. Marey and Blot, he says, assign the same cause; but he finds new proofs of its being the cause in the facts related in reference to premature confinements; to the cases where the child died before birth; and in the effect of the seasons. In premature confinement the same cause for the increased arterial tension exists as when birth takes place at the full term, and when the child dies before birth it is then the supply of blood to the uterus is diminished, and consequently it is then the pulse is lowered, and not after the child is expelled; and it is well known that cold increases the arterial tension, and the lowering of the pulse is most marked in the cold seasons.

The analysis of four hundred observations gives ninety-four cases in which the rhythm of the pulse was altered; most frequently it was irregular and uneven at the same time; exceptionally it was irregular or unequal only. Dr. Hémey has met with these phenomena but twice before labour, and in both cases the patient had old organic disease of the heart. In all the other cases the irregularity showed itself from the first to the tenth day after labour, with the exception of three in which it did not appear until the fifteenth or eighteenth day; but it is to be noted that very few of the observations extended beyond the twelfth day, so that the extreme limit of the occurrence has not been fixed. In some cases the irregularity disappears quickly, but may return soon, and it is rare to find it persist more than six days without interruption. The irregularity and inequality may accompany one another, or exist separately, and these modifications always supervene very rapidly on one another. Of the ninety-four cases, the irregularity without inequality was only observed in twelve, and the inequality without irregularity in eight; very often the irregularity and inequality exist with the slowness of the pulse, but the slowness may be found without the others, and they may occur with slight acceleration of the pulse.

The prolongation of labour does not of itself seem to influence these phenomena, but when it causes any injurious consequences the irregularity disappears. It is in fact rare to find a pulse febrile and irregular. There does not seem to be any relation between

these phenomena and the secretion of milk, except that milk fever, like all other febrile conditions, will cause them to disappear; neither does the term of pregnancy, nor the state of the life or death of the child seem to produce any effect; nor do moral emotions, except so far as by quickening the pulse they cause irregularities to disappear. Pursuing the investigation under the same heads as in reference to the slowness of the pulse, Dr. Hémey does not find that the diet or age of the patient, primiparity or multiparity, nor yet the season of the year exercises any influence; nor does he think the presence of these phenomena indicates anything unfavourable as to the recovery of the patient; and he is unable to suggest any cause for them beyond some obscure condition of the nervous system.

PUERPERAL TEMPERATURES.

It is to be regretted that this subject has not received the attention it deserves, but the following extract from Mr. Squire's paper in Vol. ix. of *Obstetric Transactions* are of interest, especially in connexion with those of Dr. Hémey on the pulse. The observations were made chiefly in the axilla.

"The temperature of the body is somewhat increased during pregnancy, at least in the latter months. The normal temperature may be taken at 98°·75, the temperature after the sixth month of pregnancy will be frequently found somewhat over 99°. A variation is to be observed in different persons, and in the same person in different states of health; the limits of this variation, as consistent with health having been restricted in the cases I have noted, to one degree, viz., from 98°·8 to 99°·8, its range being less than in women of similar age when not pregnant. Some remarkable oscillations of temperature, such as we need not expect to find in pregnancy, have occurred in a few cases that have presented themselves to me for notice, which tend to show that there is a considerable fall in temperature on the occurrence of the catamenia, and a variable rise shortly before: in one instance the vaginal temperature was found to be 99°·8 on the day before the catamenial flow, and only 97°·3 on the day after. No observation was obtained on the intermediate day; the flow was not very free, and was unaided by alcoholic stimulants, indeed, these had been abstained from for nearly a month, until this day, when they were resumed; three days afterwards the temperature was 98°·7, the period being then over: it had been in every respect normal, yet there was a fall of 2½ degrees in the first two days of its occurrence. In another case, where a vaginal operation was performed on the day preceding the expected period (which recurred with perfect regularity), the local temperature was 100°·1;

eight days afterwards, though there was some persistent local irritation, the thermometer did not go beyond 98°·7. There may be considerable pain, tenderness, and sensation of heat, without the thermometer being much affected; in a case where these conditions were all present from the too free application of undiluted liquor plumbi to verrucæ of the labia minora and fourchette, the local temperature was only 98°·9, and even this was accompanied by a slight increase in the general temperature as taken in the axilla.

"The difference between the vaginal and axillary temperatures will seldom be more than one-third of a degree, and frequently only one-fifth or one-tenth, if in the latter situation all the requisite precautions to secure accuracy are observed. In this way 98°·45 was obtained as the normal temperature in the axilla, and 98°·75 in the vagina, where there was no disturbing circumstances; in a case of abortion at two months, the vaginal temperature on the ninth day was 98°·4, the axillary temperature 98°.

"A comparison of these differences was made in the puerperal state; first, on the second day of delivery where slight perineal fissure existed, the temperature there was 98°·5, in the axilla 98°·3; so that a local injury had not disturbed the usual relative temperatures: second, on the fifth day of delivery, where the breasts were very full and tender, the milk increasing and the lochia decreasing, the axillary temperature had risen to 100°·3, the vaginal temperature was 100°·7; so that while in full activity the mammary gland does not, by its proximity to the axilla, unduly influence the results there obtained by the thermometer: in the third case, on the ninth day, a final application was required to complete the cicatrization of a perineal fissure, the temperature there was 98°·3, that at the axilla 98°·2; in this latter situation there was probably now less difficulty in obtaining the true temperature, on account of the process of lactation being in full activity.

"The commotion and efforts of parturition itself, while confined within the ordinary and safe limits of natural labour, cause but a slight elevation of temperature. In the twelve cases, of which tabulated diagrams accompany this paper, this point was made the subject of exact observation in cases 1 and 9; it had previously attracted my attention in a case of prolonged first labour, which came under my notice last autumn, where there was a great heat of surface, thirst, and remarkable dryness and heat of the passages, but no corresponding effect upon the thermometer; the patient had passed a night without sleep, and the greater part of the day without food: egg and milk, and a liberal supply of beef-tea, enabled the second stage of labour to be completed favourably. In case No. 1, on the diagram, the labour was more rapid, but of some severity, with considerable agitation, heat, and perspiration, and the development of the passages proceeding rapidly; the axillary temperature could not be obtained at even the usual standard; there was,

however, a progressive increase shown by the thermometer used in the vaginal examinations; the highest reading, obtained half an hour before delivery here, was 99°·9, in the axilla only 97°·8. In case No. 9, where the labour was more rapid, but much less severe, the temperature per vaginam, two hours after the commencement of labour, was 98°·9; in a similar observation taken, taken twenty minutes before the expulsion of the child, it was 99°·1, and ten minutes after, exactly 99°.

"The elevation of temperature thus occasioned, is maintained to the termination of labour; then commences a steady decline, which in most cases, if not all, has reached the normal line and in some has descended considerably below it. This subsidence always takes place in the first twenty-four hours; it may, however, be prolonged into the second day, as in Case 9, or be accomplished within the twelve hours, as occurred in Case 6. In this case delivery took place at 3 a.m. with some hemorrhage, sleep followed and the mammary secretion was rapidly established. In Case 9, a short labour terminated at 11 p.m., with no loss of blood, and followed by excitement rather than sleep, the lowest point was not reached until the second night, thirty hours after delivery. In Case 4, delivery at 7 a.m.; the temperature at 2 p.m. was 99°·4, she had not slept, had only taken gruel, she herself felt cool and rather faint; after sleep the temperature soon subsided to the ordinary degree. A difference is noticeable in following the line of temperature in this case, it is the only one of the series in which suckling was not attempted. In Case 1, the high temperature noted during labour had fallen four hours afterwards to 99°·2; the diagram in this case gives the simplest form of disturbance of temperature common to the puerperal state.

"In all the cases here investigated, the most constant and most obvious disturbance of temperature is that which ushers in, and accompanies, the formation of milk. The commencement of this reaction is most regular; it attains a certain prominence forty-eight hours after the birth of the child. In only one of these twelve cases was this rise of temperature retarded, in only two was it accelerated. The termination of this reaction is most variable; the modification which the line of temperature now undergoes from the merest accidents, before it settles down into the normal line at the period of convalescence, fully warrants the care and watchfulness at this time traditionally conceded.

"Where the secretion of milk is readily established, the rise in temperature necessary to its formation at once experiences a considerable fall; this is shown in Nos. 1, 3, 6, and 9, where a fall of at least one degree occurs within twenty-four hours; in the three last the temperature has in that time fallen to a point as much below the ordinary temperature as it had previously been above it. In these cases there was at the same time a very free flow of milk.

"To obviate any sources of error due to diurnal oscillations of temperature, observations were taken in Case 2 twice every day, between nine

and ten o'clock morning and evening, until the fifth day, at noon on some of these days, and in the afternoon on the subsequent days; these showed a uniform progression in the direction indicated, and it was not until the puerperal state was nearly over, and the convalescence well advanced, that the ordinary diurnal variations of temperature again became evident.

"The general conclusions offered to this Society as fairly deducible from these observations are—

"1st.—That no great elevation of temperature arises in natural labour.

"2nd.—That there is afterwards a considerable fall of temperature which is favoured by sleep.

"3rd.—That there is a subsequent exaltation of temperature which has for its natural termination the secretion of milk.

"4th.—That the bringing the method of observation here followed to the study of the puerperal state, would add an element of certainty to the principles aud details of its management, and afford an additional guide for safe conduct through some of its complications."

PLACENTA PREVIA.

Dr. T. Gaillard Thomas[a] relates eight cases of this complication, and urges the rapid induction of labour.

"The dangers attendant upon the condition develop themselves most markedly in the first stage of labour, and death not unfrequently occurs before the os externum is dilated to a size not greater than a Spanish dollar. At this time surgical interference, if resorted to to accomplish delivery, often destroys the lives which it is intended to save. The hand forced too soon through a rigid os will often rupture its walls, while a delay without the adoption of the means capable of controlling hemorrhage will necessarily favour the occurrence of a fatal result.

"On the other hand, should full dilation of the os have taken place, and the patient be exhausted from sanguineous loss, the practice of rapid artificial delivery will not rarely be followed by fatal prostration.

"There is no question, in my mind, of the fact, that when it becomes the recognized practice to resort to premature delivery as a prophylactic measure in these cases, the statistics which have been quoted will be very much improved upon. By resorting to this measure we should be dealing with a woman who is not exhausted by repeated hemorrhages; the obstetrician would be in attendance at the commencement of the labour; and he would be able by hydrostatic pressure to control flooding, while the same pressure accomplished rapidly and certainly the first stage of labour."

THE RECTUM IN ITS RELATION TO UTERINE DISEASE.

Under this title Dr. Storer contributes a series of papers of very great interest, to the *American Journal of Obstetrics*. He

[a] American Journal of Obstetrics and Diseases of Women and Children, Vol. i., pp. 2, 3.

argues that the frequency of organic disease of the rectum in women is, as a general thing, under-estimated; and that the importance of such disease in exaggerating or modifying the character of uterine disease is, as a general thing, not appreciated or recognized. Dr. Storer has found the following method of examining the rectum so useful that he has long since thrown aside the anal speculum for purposes of diagnosis in women.

"By passing the finger into the vagina, and pressing backward and downward over the levator ani, the rectum can be everted through its sphincter like the finger of a glove. This can ordinarily be done to a very great degree; it can always be done to a certain extent. Should the sphincter be unusually irritable, and spasmodically contracting with violence when touched from below or thus from above, it can be forcibly distended by the thumbs, and temporarily ruptured, as I am in the habit of doing in such cases; the procedure above indicated thus becoming easy. We can in this manner ascertain the presence of chancre or chancroid, the character of polypi, the extent and number of internal hemorrhoids, the position of the inner orifice in fistula, &c., with far greater certainty and alacrity than by speculum, or can be done in the male, while the mere eversion process, provided rupture of the sphincter is not necessary, is attended by very little pain."

COCCYODYNIA.

Dr. Nott records an aggravated case of this affection cured by the extirpation of two bones of the coccyx; an operation he claims to have been the first to perform, which he has done a number of times, and always with more or less perfect relief. The subcutaneous division of the surrounding tissues, as recommended by Simpson, he has not yet tried, but he purposes to do so. He expresses his view as to the pathology of the disease in the following passage:—

"It has then been suggested that coccyodynia depends on a morbid condition of the periosteum of the coccyx, of the tendons of the muscles attached to it, of some of the small muscles themselves, of the surrounding fibrous tissues, of the nervous fibrils, and, lastly, of the *glandula coccygea;* but, so far as I am informed, we have, as yet, no reliable data for determining the pathology of this affection. If the seat is really this gland, it is a curious inquiry to ascertain whether the gland belongs alone to the female sex.

"I will venture to start another suggestion with regard to the pathology. It is a well-known law, that where any chronic irritation is brought to bear on a muscle, a strong tendency to spasmodic contraction

* American Journal of Obstetrics, Vol. i., No. 3, p. 243.

results, and acute pain is produced when any attempt to elongate the contracted muscle is made. Familiar examples of this are seen in the sphincters of the anus and vagina, from fissures of anus or ostium vaginæ, or other sources of irritation; vaginismus from irritation of the carunculæ myrtiformes; or, as sometimes happens, vaginismus from fissure of anus, or from hemorrhoids. In all these, some point of local irritation in the vicinity of the muscles excites spasmodic contraction, and any attempt at dilatation of the circular muscle gives exquisite pain. Future and more careful investigations may possibly show that the pain in coccyodynia is attributable to the spasmodic contraction of some of the muscular fibres connected with the coccyx. There is little pain except when motion is communicated to some one or more of the muscles connected with the coccyx, and in this it differs from true neuralgia.

"Division of the muscles in question, as in fissure of the anus, and in vaginismus, gives relief."

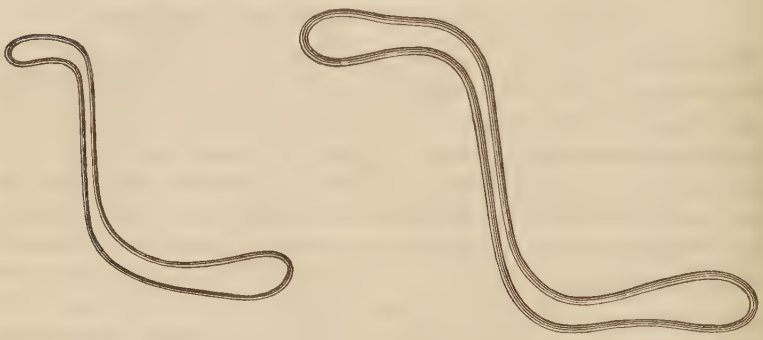
Combined Wire Speculum and Retractor. By Francis H. Brown, M.D., Boston.

"A few weeks ago, I was asked by Dr. Waterman to assist him in a vaginal operation, which required the passage to be fully shown, and indeed, was quite protracted in its performance. I took occasion to suggest employing a simple wire speculum, which would answer the additional purpose of a retractor, in place of the Sims and Boston (Storer) specula and the copper spatulæ he was proposing to use. He carried out my idea by bending a stout wire on itself, and then shaping both folds nearly into the form of the letter S; and thus got a very excellent and serviceable instrument. After I made more carefully, and, after some experiments, the two forms of the instrument which I now describe.

"They are made of iron wire, joined by hard solder and then nickelized. The wire used is numbered 10 and 13 of Stubb's (English) wire gauge. The general description of the larger instrument would be that it is a *skeleton* Sims' speculum; very nearly of the same size, and with the same curves; the two handles turning in opposite, instead of the same direction; and one blade somewhat shorter and narrower than the other. The instrument made from the smaller wire is, generally, smaller, and has one of its ends proportionately shorter; otherwise its general shape is the same.

"The objects for which I propose these instruments, are the following:—Singly, the larger instrument has been found well adapted to take the place of Sims' speculum. It displays the vagina equally well; it is much lighter; equally strong; and no less easy of application. The two together, as retractors or in place of the copper spatulæ, have served well to dilate the vagina, and bring into view every part of its wall, up to the cervix. Of course, this use pre-supposes one or more assistants;

but, when so employed and guided by the eye of competent helpers, they fill, more satisfactorily than the bivalve and multivalve specula, the place of these instruments, while they are more fully out of the way of of the operator. They have, moreover, the additional advantage over the common copper spatulæ, that they have no sharp edges.



"Singly the instrument may, not unfrequently, be used to hold and render tense the vagina, and, by its fenestrum, bring into clear view and make prominent an abscess or small tumour, an ulcer or fistula, on which one desires to operate, much as the fingers of the operator or assistant would do on the external surface of the body. It may, not inaptly, be employed after rupture of the sphincter ani for the display of and operations on internal hemorrhoids and ulcers of the rectum, or the internal opening of a fistula in ano. The retractor will be found a useful aid in searching, by means of its fenestrum, for vesico-vaginal fistulæ—which, as is known, sometimes baffle the search of the surgeon for a considerable time.

"The smaller instruments are to be used especially as retractors, to hold aside the edges of wounds during various operations for the removal of tumours, ligature of arteries, &c., and to keep out of danger arteries, veins, nerves, and similar important parts. The smaller retractor answers the purpose of the wire tongue depressor, devised by Dr. Allen, of Cambridge, and serves as an admirable retractor to draw aside the cheek or lips in various operations about the mouth and maxillary bones.

"A still farther advantage of these instruments is in their very trifling expense; the whole set of four being furnished by our instrument makers at a sixth part of the price charged for any of the elaborate forms of specula."

HALF-YEARLY REPORT ON PUBLIC HEALTH.[a]

By Charles A. Cameron, Ph.D., M.D., L.K. & Q.C.P.; Prof. of Hygiene in the Royal College of Surgeons; Lecturer on Chemistry in Steevens' Hospital Medical College, and the Ledwich School of Medicine; Analyst to the City of Dublin.

WATER IN RELATION TO THE PUBLIC HEALTH.

Within the last twenty years great attention has been given to the subject of the water supplies of towns. During the middle ages almost every European town was supplied with water derived from superficial wells situated within the urban limits. At the present time, when an elaborate system of sewerage works conveys the waste matters of towns into rivers or the ocean, the water of town wells and pumps is often, nay, generally, loaded with dangerous organic matter. We may therefore easily imagine how impure must have been the water obtained from town wells in mediæval ages, when the soil was saturated with those waste animal and vegetable matters which in modern times are happily got rid of by subterranean conduits. In former ages the mortality of towns was frightfully in excess of that of country districts. In many instances the death-rate of a crowded city was more than double that of rural districts situated under the same climatic conditions. Narrow streets, overcrowded and ill-ventilated tenements, defective provision for the removal of effete matters, and personal uncleanliness, were prime causes of the shortness of human life in mediæval times—indeed, in towns long after that gloomy period of European history had passed away. But a prime factor in the causes of the unhealthiness of towns was, we believe, the bad water which the town wells supplied. When fearful epidemics decimated the inhabitants of cities the cry often arose that the wells were poisoned. This complaint was in general only too true; but the poison in the wells was not put into them designedly—it was derived from the foul drainage of the soil, tainted with the matters thrown out from the bodies of the sick. We feel a strong conviction that in former times one of the most active agents in spreading epidemic and endemic diseases amongst crowded centres of population was impure potable water.

During recent years matters with respect to supplies of water for domestic purposes in town and country appear to be very much

[a] The author of this report will be glad to receive any books, pamphlets, or papers relating to hygiene, dietetics, &c. They may be forwarded through the agencies of this Journal.

reversed. Formerly the citizen, as a rule, was obliged to drink water of bad or doubtful quality, whilst the denizen of country parts could easily get his draughts of the pure element from crystal spring or sparkling rivulet. Now, the inhabitants of the larger towns are gradually giving up the use of pump water, and are substituting therefor pure water obtained from distant lakes and rivers. On the other hand, we fear, that owing to a variety of causes, the water used for domestic purposes is not, in general, as pure as it was in the middle ages. In the first place, the rural population has greatly increased, and there is, consequently, more effete animal matter thrown into the soil. Secondly, tillage husbandry, which includes the application of large quantities of manure to the land, has been largely extended. It may be that the potable water in use in country districts is not greatly inferior to that used in the middle ages; but that there is some deterioration in its quality is certain: and this fact is evident, that most of the large towns are now supplied with better water than is obtainable in most villages and in a large proportion of detached country-houses.

The drainage of the mere surface of the ground may be very pure or the reverse, according to the nature of the soil, the density of the population, the state of the water-shed, and the provisions for the removal of organic refuse. In every place where a dense population exists, it is almost physically impossible that the surface water which supplies the superficial wells could be pure. In country districts this kind of water is occasionally very pure, owing to the absence of manure, or because the drainage is obtained from a thin and grass-covered soil. The deeper the well is the greater is the drainage area which supplies it. A shallow well often collects only the drainage of a few hundred square yards; in which case the liquid, not having been subjected to prolonged contact with the purifying clay, is almost certain to contain organic impurities in excess. When making a well these points should be fully considered. If the well be only six or eight feet deep, care must be taken not to have a cesspool or manure heap, on any part of the catchment area, for otherwise some oozings from the manure would be sure to get into the well. For this reason, if for no other, liquid manure tanks and cesspools should be so constructed as to prevent the leakage of their contents. They should also be placed as far as possible from the well—an observation which applies with equal force to sewers.

The quality of the water used in the majority of Irish small towns and villages is very bad. The "parish pump" collects not

only the rain water which falls on the catchment basin of the district, but also no inconsiderable amount of the liquid refuse thrown out from the houses and stables of the town. Many specimens of this kind of water have come under our own observation; and in several cases we were quite certain that they had been the means of spreading cholera when that disease was last epidemic in Ireland. Quite recently we examined for Dr. Tufnell the water supplied by one of the public pumps at Dalkey township, county of Dublin, and found it to contain the large amount of 70·26 grains of solid matter per gallon, of which no less than 22·06 grains consisted of "organic and volatile matters." These latter consisted chiefly of nitrates and nitrites, and therefore afforded the clearest evidence that the water had been largely contaminated with sewage impurities. Here we have the case of a well water obtained in a granite district, which in general yields exceedingly pure water, but which, owing to contamination from the refuse thrown out from human habitations, had lost all the characteristics of the water from primitive rock formations.

Mr. Bailey Denton, the eminent engineer, has suggested a method of supplying villages and small towns with good water and in sufficient quantities. He proposes to collect in reservoirs all the drainage from the subsoil when it has penetrated to the extent of about four feet. By this operation the land would be deprived of its excess of moisture, and reservoirs of very soft and wholesome water would be collected for the use of the villagers or townsfolk. In the case of a village containing 400 inhabitants, and requiring for each individual a daily supply of 10 gallons, a reservoir of $7\frac{1}{2}$ feet deep, and covering $\frac{4}{10}$ths of an acre, would be required. This pond would contain 720,000 gallons—a quantity which, making allowance for evaporation and waste in other ways, would afford 120 days' supply, without being replenished. The cost of a reservoir of this size is estimated by Mr. Denton at £415. Sanitary engineers and medical men have highly commended Mr. Denton's scheme. We fear, however, that in seasons of drought and heat the water of such shallow ponds as those proposed by Mr. Denton would become greatly deteriorated in quality. Perhaps it would be a better plan were a good water-shed selected sufficient to supply the wants of a number of small towns and villages. The Corporation of Manchester, who have a magnificent water reservoir, supply villages twenty miles distant from the works at charges considerably under the cost of the water supplies provided by Mr. Denton's method.

The Commissioners of the smallest town or township, and, in the case of purely rural parishes, the Board of Guardians of the poor have the power of providing supplies of water for the inhabitants residing within their jurisdiction. The eleventh section of the Sanitary Act of 1866 confides this power to the above-named bodies, but it does not empower them to purchase land except by agreement, and in the manner prescribed by the Local Government Act of 1855. It is unfortunate that the latter Act does not extend to Ireland, for several of its provisions affect more or less the public health. The Corporation of Dublin are urging the Government to bring in a measure for the extension of the Act to Ireland; and we have no doubt but that the request will be complied with, and probably during the present session of the Legislature.

We have already stated that the water supplies of many of the larger cities are now derived from lakes and rivers. These are, in general, situated in thinly-peopled districts, where the waters are not liable to be tainted with sewage impurities. With few exceptions, the new water supplies contain very little solid matter in solution, whilst the water formerly in use was, as a general rule, *hard*—that is, it contained from ten to fifty grains of solid matters per gallon. When the bills for the better supplying of Manchester, Glasgow, Dublin, and other towns with water were before Parliament, the medical and scientific evidence educed was nearly altogether in favour of soft water as against hard water. It was to a great extent on the faith of this evidence that Parliament was induced to sanction the expenditure of several millions of pounds for the construction of water works. After all this it is rather startling to find a high sanitary authority deliberately stating that the use of soft water in a town greatly increases the mortality of its inhabitants! The authority to whom we refer is Dr. Letheby, medical officer of health for the city of London. In his report on the sanitary condition of London during the last year, he gives a table showing the average composition of the water supplied to the metropolis during the year, and from which we learn that the proportion of solid matter per gallon varied from 18·18 to 27·85 grains. The solid matter consisted chiefly of lime salts, which Dr. Letheby contends render the water wholesome. He says:—

"In this country it is remarkable that wherever soft water is supplied to the people the mortality is large, even when allowance is made for the birth-rate of the place. Glasgow, for example, as well as Preston, Dundee, Sheffield, Plymouth, Manchester, Bradford, &c., which are all

supplied with water of less than four degrees of hardness, have a mortality which ranges from 26 to 34 per 1,000, while at Birmingham, Bristol, Sunderland, Newcastle-on-Tyne, Wakefield, Dover, Norwich, Croydon, Worcester, Derby, and other places where the waters are hard, the mortality is considerably less—in fact, it may be said that in towns supplied with water of more than ten degrees, the average mortality is about 22 per 1,000; while in those supplied with softer water it is about 26 per 1,000."

Dr. Letheby states that the *savans* appointed by the authorities of Paris and Vienna to decide on the quality of the water to be supplied to those cities, reported in favour of water of moderate degrees of hardness. One consideration seems to have powerfully influenced the decision of the French *savans*—namely, the allegation that more conscripts are rejected in the soft water districts, on account of imperfect development and stunted growth, than in the hard. "And if," says Dr. Letheby,

"We regard the subject from another point of view, there is the striking fact that, as three-fourths of the earth's surface is covered with calcareous strata, we are compelled to drink calcareous water, as if it were intended for a wholesome purpose; and it may well be, as the late Professor Johnston observed, that 'the bright sparkling hard waters which gush out in frequent springs from our chalk and other limestone rocks are relished to drink, not merely because they are grateful to the eye, but because there is something exhilarating in the excess of carbonic acid they contain, and give off as they pass through the warm mouth and throat; and because the lime they hold in solution removes acid matters from the stomach, and thus acts as a grateful medicine to the system. To abandon the use of such a water, and to drink daily in its stead one entirely free from mineral matter, so far from improving the health, may injure it. In fact, the water of a country may determine the diet of its inhabitants. The soft water of the lakes of Scotland, for instance, may have had much to do with the use of brown meal; and but for the calcareous waters of Ireland the potato could not have become a national food. Looking, therefore, at the plain teachings of all this, and considering the excellent quality of the water supplied to this metropolis, it would be folly, in my opinion, to change it for a soft water."

The citizens of Dublin have recently expended nearly half a million sterling in supplying themselves with water of about two degrees of hardness instead of the water of from fifteen to forty-two degrees of hardness formerly in use in that city. If Dr. Letheby's view be correct, the inhabitants of Dublin may justly complain that, acting upon the advice of scientific men, they have been led

to worse than useless extravagance. To our mind, however, it seems inconceivable that the use of pure soft water could be the means of largely increasing the death-rate of a town. The ordinary food which we use contains abundance of mineral substances, with which the bony structures of the body are nourished. Even if they did not, the quantity of earthy matter taken into the body through the medium of water would be perfectly inadequate to subserve any useful purpose. Let us take the case of an average sample of the hard water supplied to London. It contains 16 grains of earthy salts per imperial gallon (70,000 grains weight, or 160 ounces). Few persons drink more than half a gallon of water per day, so that a person consuming that quantity of water would derive from it only eight grains of earthy salts. It must be borne in mind, too, that by heating water, the hardness of which is due to the presence of calcic carbonate (carbonate of lime), a large proportion of the earthy salt is rendered insoluble. Much of the lime contained in the hard water used for domestic purposes is deposited in the tea-kettles and other appliances of the household. The quantity of calcic carbonate in a man of 160 lbs. weight is about one pound, and an adult daily consumes in his food at least $\frac{1}{4}$ oz. of earthy salts. As the lime contained in the bones is associated nearly altogether with phosphoric acid and *not* with carbonic acid, it is evident that the chalk contained in hard waters cannot contribute, at least in an important manner, to the nutrition of the body.

The apparent relation between the death-rate and the nature of the water supplies in the towns mentioned by Dr. Letheby is certainly remarkable; but we should like to learn the death-rates in Glasgow, Manchester, and the other towns supplied with soft water when the water furnished to them was hard. Dr. Letheby includes Dublin amongst the towns which have a very high death-rate; and yet, until very recently, that city was supplied with water of about the degree of hardness which Dr. Letheby considers most desirable. So far as our observation extends, we are clearly of opinion that the substitution of the pure soft water of the Vartry for the hard water supplied by the canals, has produced an improvement in the public health.

We have received a letter upon this subject, from Dr. Gairdner, medical officer of health for the city of Glasgow, and author of an excellent work on air and water. We quote the following extract from Dr. Gairdner's letter:—

"The highlanders and islanders of Scotland—who despite many unfavourable circumstances—enjoy the credit of a low death-rate, almost all drink soft water; and I do not know a single fact showing that soft water, in the many instances in which it has now been brought into populous places, instead of hard, has in any instance led to an increased death-rate, or produced any appreciable bad effect where the other sanitary circumstances are good.

"In Glasgow the whole of the west-end well-to-do population drink Loch Katrine water, and do not suffer. The death-rate of the town is affected by quite other causes."

PROPAGATION OF CHOLERA AND TYPHOID FEVER.

At a meeting of the Association of Medical Officers of Health, London, held on the 19th February, 1870, Dr. George Buchanan read an elaborate paper on Pettenkofer's theory of the propagation of cholera. The paper is published *in extenso* in the *Medical Times and Gazette*, March 12th, 1870. In our Report on Public Health, published in the *Quarterly Journal of Medical Science*, November, 1869, we fully described Pettenkofer's newest views relative to the propagation of cholera. The correctness of these views is disputed by Dr. Buchanan, who, from his position of Government sanitary inspector, has had peculiar facilities for investigating the conditions under which cholera spread in England during the recent epidemic of that disease. Dr. Buchanan believes that cholera and enteric fever are communicated chiefly through the medium of polluted water. Pettenkofer states that when the soil water is lowered the cholera poison diffuses itself throughout the porous soil: but, according to Buchanan, when the soil water subsides, the superficial wells become more impure from effete animal matters; and the drinking water which they supply is the direct carrier of the disease. It is evident that when the soil water is nearly up to the surface of the ground the waste matters thrown out from dwellings are carried off horizontally by the drainage; whilst, when the soil water is low, the refuse sinks into the surface-wells, which thereby become polluted, and during epidemics of cholera, or endemics of enteric fever, retain the virus of those diseases. Dr. Buchanan quoted several facts to prove that typhoid fever is spread by means of polluted potable water; and he endeavours to prove that the subsidence of soil water does not affect the spread of the disease in localities supplied with water brought from pure and remote sources. We quite agree with Dr.

Buchanan in regarding impure drinkable water as a common vehicle in which the germs of several zymotic diseases are carried into the body. During the last epidemic cholera in Ireland we found that the pump water used in several localities where the disease was unusually severe, contained excessive amounts of animal impurities. This was particularly the case with Arklow and Mallow. In the latter town there were few cases of the disease except in one suburb, the well water of which we found loaded with organic matter, nitrates, nitrites, and ammonia. Where the disease did not prevail the water was remarkably pure. Dr. Buchanan's views are greatly strengthened by the facts set forth in Ballot's report on the spread of cholera in Holland; for that author has shown that in every town in the Netherlands, where rain water alone was drank, there were either no cases of cholera or a few isolated ones. On the other hand, where wells and canals furnished the water of towns or villages, the disease prevailed. Pettenkofer himself does not deny the fact that cholera and typhoid fever are occasionally contracted by drinking impure water. In November last we had the pleasure of conversing with Professor Pettenkofer upon this and other subjects, and he stated that he believed cholera was occasionally communicated through the agency of potable water, but that the general spread of the disease was not through that channel.

The most striking fact adduced by Dr. Buchanan against the most important part of Pettenkofer's theory is that, whilst main drainage works were carried on in seven large towns the mortality from enteric fever actually declined. These towns were supplied with water from a distance, and therefore the subsidence of the drainage only affected the wells, from which no important part of the water supplies of the town was derived.

"The following shows the mortality from enteric fever in each town before and during the years when the soil water was sinking, the figures representing in each case a death-rate per 10,000 of population yearly:

	Before.	During.
Bristol, - - -	10	8
Leicester, - - -	15	$10\frac{1}{3}$
Merthyr, - - -	$12\frac{2}{3}$	$8\frac{2}{3}$
Cardiff, - - -	$17\frac{1}{2}$	$4\frac{1}{2}$
Carlisle, - - -	10	$8\frac{3}{4}$
Macclesfield, - -	$14\frac{1}{4}$	14
Newport, - - -	13	$12\frac{1}{4}$

Of these towns the lowering of soil water was most considerable in the cases of Leicester and Cardiff. It may be well that I should guard against misapprehension by stating that, in the period after the execution of the works here considered, the reduction of mortality from enteric fever was, with scarcely an exception, greatly more conspicuous than in the time during which the works were being done.

"It appears, then, from the experience of these seven large towns, that when lowering of soil water is brought about, while drinking water is being supplied, not from the soil of the place, but from pure external sources, Pettenkofer's experiences, obtained from places which do derive their water supply from their own soil, fail of application. It is probable that if closer inquiry were made into the sequence of sanitary improvements, some or all of the towns which were first set aside might afford further evidence adverse to the general expression of facts given by Pettenkofer—at all events, none of these towns afford any colour to the supposition that sinking of soil water exercised appreciable evil effect against the good effects that were being gained in other ways.

"The three towns which I have left for further mention are Croydon and Worthing, where during the years of soil water reduction there was an increase of fever; and Stratford-on-Avon, where, with considerable reduction in the level of soil-water, great reduction of fever also took place, and this although the source of water supply remained what it had always been—the porous ground upon which the town stands. The case of Stratford is the only one which appears to me antagonistic to the whole of Pettenkofer's theory. Its fever death-rate was $12\frac{1}{3}$ yearly before drainage works were begun in 1854. The construction of the sewers reduced the level of the subsoil water, intercepting springs, and drying wells, many of which had to be deepened. Yet, while these works were going on, the annual fever death-rate was only $5\frac{3}{4}$. The other two instances, Croydon and Worthing, appear at first sight to bear out Pettenkofer's views in their entirety, for both of them, having pure water supplied to them from sources out of the reach of contamination, got their soil water much reduced during the time that certain drainage works were being made, and at the same time got a considerable increase of fever; in the case of Croydon from $16\frac{1}{2}$ to 19 per 10,000, and in the case of Worthing from $7\frac{1}{2}$ to $9\frac{1}{2}$. But in each of these places it is known that arrangements intended for the removal of excrement had for their result the free discharge of sewer gas into houses, and the evidence connecting the outbreaks of enteric fever with this defect is so conclusive as to obscure altogether any causative conditions relating to the soil.

"Admitting, then, Professor von Pettenkofer's thesis that subsidence of soil water is a condition favourable to the epidemic prevalence of

enteric fever, I think it has been shown that the qualification is wanted 'where in the town or place the supply of drinking water is derived from the soil upon which the town stands;' and then the conditions to which Pettenkofer calls attention will range themselves under the more general etiological head of circumstances assisting in defiling supplies of drinking water. I have not much doubt that what is true, in this regard, for enteric fever, is also, in its degree, true for cholera; but upon this point, remembering the varied character of the statements connecting cholera with soil, and feeling the need of wider and more exact experience, I cannot venture to speak with certainty."

Dr. Bryden, of the Bengal army, has recently published a voluminous report on the outbreak of cholera in Northern India in 1866-68. Some of the facts mentioned in this report are strongly in favour of the theory of the general propagation of cholera by means of impure water. Whilst the water-tanks are fouled by dipping clothes into them and bathing in them, and the supplies are drying up, the disease increases rapidly; but when the tanks are cleaned out and refilled with pure rain water the cholera vanishes, and does not reappear during the time that rain keeps the tanks full of water.

DUST AND DISEASE.

Professor Tyndall, in a lecture delivered in December last, has popularized a subject which previously was known only amongst scientific circles. The professor, in making some experiments with beams of light, found that the dust present in the air interfered with the results. This dust proved to be chiefly organic, and therefore combustible, and he destroyed it by passing the air through a platinum tube heated to redness. Professor Tyndall also noticed that the air passing into the mouth in the act of inspiration contained dust, whilst the air expired from the lungs was free from solid particles. As cotton wool absorbs and retains atmospheric dust when a current of air passes through it, Dr. Tyndall recommends persons who are obliged to be in contact with patients suffering from contagious diseases to wear respirators made of cotton wool. This advice is, of course, based on the germ theory of zymotic diseases; and it presumes that matters containing the seeds or virus of the disease are exhaled from the lungs and skin of the affected. Dr. Tyndall causes a ray of light to traverse the air. If it be visible to the ordinary observer it is because

germs of animals and plants (and perhaps of diseases), or other solid matters are present. Where the air is free from solid matters, there the sunbeam—or rather what is popularly termed the sunbeam—is not visible. In ventilating buildings by forcing a current of warm air into them, would it not be advisable to subject the air to a temperature sufficiently high to destroy any organic matter which it might happen to contain? In heating and ventilating such buildings as the Houses of Parliament, where expense is not considered, it might be found practicable to employ highly heated air, subsequently cooled to the proper temperature for breathing it.

Although the interesting lecture of Dr. Tyndall has directed general attention to the presence of organic matter in the air, the subject is by no means either new or but little studied. In one of our lectures on Public Health, delivered in May, 1868, in the Royal College of Surgeons, we exhibited the presence of organic matter in the air by passing the latter through a solution of permanganate of potash. In a few minutes the atmospheric organic matter decomposed the permanganate solution, the splendid red colour of which gradually faded into a yellowish hue. In a little work[a] of ours on Health, the following observations occur:—

"The organic matters in the atmosphere consist of a great variety of bodies. They comprise seeds, spores, pollen, minute vegetables, infusoria, insects and their remains, soot, fragments of linen, cotton, silk, and wool, hairs, particles of animal and vegetable substances. Minute traces of matters thrown off from animals exist in the air, and constitute the most dangerous part of the atmospheric organic matter. Pus has been detected in the air of hospitals, and epithelium from the skin may be found in the dust of most inhabited rooms. Chalvet collected the dust in a badly cleaned and ill-ventilated hospital, and found it to contain from 33 to 46 per cent. of organic matter.

"*Table showing amount of Organic Matter in Air.*

	Cubic Feet.
Pure air on high ground, 1 grain in from -	176,000 to 209,000
In a bed-room, - - - - -	„ 56,000
Inside a house, - - - - -	„ 16,000
In a closely packed railway carriage, - -	„ 8,000
Air of a cess pool, - - - - -	„ 62

[a] Lectures on the Preservation of Health (pages 47 and 48). London: Cassell, Petter, and Galpin. 1869.

"The experiments of Dr. Angus Smith clearly prove that the headache and other symptoms which we suffer from in ill-ventilated places are nearly altogether the result of the organic matter, and not of the carbonic acid, of the vitiated air. Hammond found that a mouse died in 45 minutes in air containing a large amount of organic matter, but no carbonic acid. Other experiments have afforded similar results."

At a meeting of the London Chemical Society, held on the 3rd February, Mr. Chapman read a "Note on the Organic Matter contained in the Air." The author found that the air of crowded rooms contains suspended nitrogenous organic matter as well as volatile organic bases. The first is removable by simple filtration through cotton wool; but the latter pass through filters, and when conducted through water can be detected therein. The air collected from the neighbourhood of a sewer contained decided quantities of these bases. Mr. Chapman suggests that it would be desirable to examine by this method the air of hospitals, fever wards, and similar places.

At a meeting of the Manchester Literary and Philosophical Society, held February 22, 1870, Dr. Arthur Ransome read a paper "On the Organic Matter of Human Breath in Health and Disease." The breath of eleven healthy and of seventeen persons affected by different diseases was examined. In both health and disease free ammonia was exhaled. Urea was sought for in the breath of three healthy persons and twelve diseased. It was found in two cases of kidney disease, in one of diphtheria, and but in the minutest quantity only in the case of a female suffering from catarrh.

A rough calculation was given of the total quantity of organic matter exhaled from the lungs in twenty-four hours. In the case of an adult it amounted to about forty-six grains. In kidney diseases a very large amount of organic matter was thrown off from the lungs. In a case of diphtheria confervoid filaments were observed; and in two other cases—one of measles, the other of hooping cough—numerous specimens of a small celled torula were detected.

The results of Dr. Ransome's experiments throw no light on the subject of the germ theory of disease. They show, however, the readiness with which the matters thrown out from the lungs support fermentation, and they afford a further illustration of the dangers arising from the ill-ventilation of hospitals. In concluding

this notice of the subject of Dust and Disease, we cannot but remark that Dr. Angus Smith of Manchester has done far more than any other chemist to elucidate the nature and to determine the amount of atmospheric organic matter in town and country, indoors and out of doors.

REGISTRATION OF DISEASE AND DEATH.

Last November a deputation, consisting of a conjoint committee of the Medical Society of London, the Association of the Medical Officers of Health, the St. Andrew's Medical Graduates, and the Poor Law Medical Officers Association, waited on Mr. Goschen, the president of the Poor-Law Board, to submit to him the basis of a scheme for the registration of disease. The plan suggested proposes that the usual weekly returns of illness made out by the ordinary poor-law medical officers, should be applied in such a way as would render them available for national, instead of purely local purposes. Each dispensary physician makes a weekly return to his board of guardians; a copy of this return might be forwarded to a central office—for example, to the office of the Registrar-General—where the facts contained in it, and other similar returns, would be classified, and published with reports upon them conveying valuable information to the local health authorities, and to the general public.

If a system of disease registration were initiated by the poor-law authorities, statistics of disease would very soon follow from hospitals, infirmaries, asylums, private dispensaries, medical relief associations, and from other sources: even private practitioners might ultimately be induced to co-operate in securing a really general national system of disease registration, which would be invaluable as a means of investigating the etiology of the zymotics, and perhaps of other maladies. The president of the poor-law board, Mr. Goschen, entertained very favourably, the proposal to establish a system of disease registration; and although it has not as yet been adopted, we have little doubt but that the practicability of the proposed plan will ere long be properly tested.

Mr. Watt, of Montreal, has in a recent pamphlet endeavoured to prove what he terms the fallacies of the Registrar-General's conclusions, with respect to the mortality tables of towns. He shows that the mere death-rate of a place is no criterion as to its salubrity, unless corrected by means of factors taken from the birth-rate. The immigration into, or the emigration out of, a town also affect

the conclusions deducible from the mortality tables. Dr. Letheby also attacks[a] the Registrar-General. He states—

"In the weekly and quarterly returns of the Registrar-General, the death-rates of certain British and continental towns and cities are so prominently stated, that the reader is apt to conclude they are the direct expressions of their sanitary condition. But nothing can be more fallacious; for the town with the lowest death-rate may be the most unhealthy, and so also conversely. To take, for example, the list of places referred to by the Registrar-General in his quarterly report, it would seem that Dublin with a death-rate of 22·72 per 1,000 of the population, must be considerably more healthy than Liverpool with a mortality of 27·29 per 1,000. In reality, however, it is the reverse, for if measured by the birth-rate as well as the death-rate, it will be found that Liverpool is in a better sanitary condition than Dublin. Taking, in fact, the average birth-rate (35·08 per 1,000), and the death-rate (22·58 per 1,000) of England as the standards for comparison, it will be seen that the actual death-rates of these places during the year which has just expired are very different from the calculated death-rates; and that little or no estimate can be made of the comparative salubrity of these places by mere reference to the observed death-rates.

Observed Annual Birth-rate and Death-rate per 1,000 *of the Population, and the calculated Death-rate.*

	Annual Rate per 1,000 Living			
	Births	Deaths	Calculated Death-rate	Difference of actual Deaths
Birmingham	35·65	20·35	22·94	— 2·59
Bristol	36·09	22·64	23·23	— 0·59
Leeds	40·86	27·26	26·30	+ 0·96
Sheffield	3·683	25·94	24·86	+ 1·08
Newcastle-on-Tyne	38·88	26·27	25·02	+ 1·25
London	35·48	24·13	22·83	+ 1·30
Bradford	37·94	26·03	24·44	+ 1·59
Salford	39·47	27·67	25·41	+ 2·26
Liverpool	36·93	29·07	23·77	+ 5·30
Manchester	35·91	29·15	23·11	+ 6·04
Edinburgh	37·64	30·63	24·23	+ 6·40
Berlin	41·78	33·61	26·89	+ 6·72
Dublin	25·93	23·81	16·69	+ 7·12
Glasgow	40·11	34·23	25·82	+ 8·41
England (average)	35·08	22·58	22·58	0·00
Austria „	40·45	30·85	26·63	+ 4·42
France „	26·68	23·27	17·17	+ 6·11

[a] Report on the Sanitary Condition of London for the year 1869. London: Lownds, Fenchurch-street. 1870.

"In the above list the towns have been placed in the order of their salubrity, as determined by a comparison of the observed and the calculated death-rates, beginning with the most healthy; and it will be remarked that the observed death-rate is always very different from the true death-rate—that Dublin, for example, which has an observed death-rate of only 23·81 per 1,000, is nevertheless an exceedingly unhealthy city—much more so than Leeds, with an observed death-rate of 27·26 per 1,000; for the latter, considering the number of children in the place, has a death-rate but slightly above the estimated death-rate (26·03), while the former with its small proportion of children, has a death-rate excessively above it (16·29)."

It is but fair to state, that in the full reports published by the Registrar-General, he points out that fallacious conclusions might be drawn from his statistics; and he explains the necessary corrections which must be made in comparing the mortality returns of towns, with those of the rural districts. In the weekly returns published in the newspapers, these corrections are not, however, given; and consequently the inhabitants of very unhealthy towns, the observed death-rate of which may happen to be low, conclude from the Registrar-General's returns, that their sanitary state is excellent.

With respect to the vital statistics collected in Ireland, it may truly be said that they are utterly valueless. A large sum of money is annually expended in collecting statistics of births and deaths; and we cannot but express our regret that so much of the public money is wasted in gathering and publishing statistics which serve only to mislead us. Last year the death-rate in Dublin amounted to 24 per 1,000 persons, living, whilst the births registered numbered only 26 per 1,000 persons. In some former years, the birth-rate was actually under the death-rate. Every physician in Dublin must know, that in it the births greatly exceed the deaths; and that the discrepancy between the actual birth-rate, and that recorded by the Registrar-General arises from the fact, that the lower classes do not, whenever it is possible, cause the births of their children to be registered. We believe that only a small percentage of the births of children born without their mothers having received medical assistance, is officially recorded in this city.

In the rural districts of Ireland, more especially in Connaught, registration of births and deaths is much neglected. A foreign student of vital statistics, reading the official statements of the

birth and death-rates in Ireland, would be astounded to learn from them, that the average duration of human life in Connaught, was eighty years!

The registration of births and deaths in Ireland ought to be either abolished or thoroughly reformed. The law making it compulsory to register those events should be rendered more stringent. No interment should be permitted unless the persons conducting the funeral produce to the keeper of the cemetery a medical certificate showing the cause of death. The police might be employed in ascertaining recent births and deaths, and in serving cautionary notices to have those events registered. Their services in this way might be annually rewarded; the bonus being proportionate to the number of cases registered in consequence of their information.

The fee received by the medical registrar for recording a death, is only one shilling—a remuneration absurdly small, and hardly adequate to pay for the mere scrivenery connected with the registration work. The sum paid for each registration should, at least, be doubled. It has frequently been suggested that it would be desirable to relieve the dispensary doctors from their functions as registrars of births and deaths. On this subject, we had recently occasion to make the following observations:—

"To me it appears evident that no satisfactory results will flow from any system of registration that is not exclusively managed by medical men. The duty of recording the births and deaths—I will say nothing of the marriages—should be entrusted to the poor-law medical officers; who, in return for their monopoly of this function, should be obliged to perform their work in a manner that would ensure its results being valuable to statistical and sanitary science. To any one who appreciates the advantages derivable from the study of accurate vital statistics, the peculiar fitness of physicians for the office of registrars of births and deaths is obvious. They would not commit the mistakes in the nomenclature of disease which the non-medical registrars sometimes make, thereby rendering their returns worse than useless. They alone would be able to indicate the causes of an unusual amount of mortality in their districts. As they constitute more than a third of the whole body of medical practitioners, and undoubtedly have charge of more than two-thirds of the sick, they have themselves the opportunity of ascertaining, personally, a large, if not the largest, proportion of the deaths occurring in their districts.

"I am quite satisfied that it is in the power of the dispensary physicians

who are also the registrars of deaths and births, to enlarge our knowledge of the etiology of disease—the first step towards its prevention or cure. We have a great deal to learn, not only relative to the ills which flesh is heir to, but to the flesh itself. Why is it that a disease extinguishes the spark of life in one man, deals gently with another, and passes by a third? I believe that the causes of death are seldom simple, and that in the majority of cases it is a bad constitution (*diathesis*), and not his malady, that kills the sick man. No doubt, it is useful to learn how many members of the community die annually from consumption; but the information would be far more valuable if it were known in how many cases the disease was the result of following an injurious trade, or of living upon a damp soil—whether it was produced from a direct cause, or was the result, mediate or immediate, of a constitutional tendency to phthisis. In short, the object for which vital statistics are collected would be more likely to be achieved if accurate information were obtained relative to the causes, proximate and remote, of deaths. Perhaps it would not be found impracticable to fill up some such form as that shown in the diagram.

"*Particulars to be stated in Form for Registration of Death.*

"Name—Age—Bachelor—Married or Widower—Birth-place—Diathesis—Occupation—Climatic and Telluric Conditions of Residence and Place of Business—Ages at, and Diseases of, which his relatives of the first degree had died—Disease or Diseases which directly or indirectly caused the death.

"It is, of course, not to be expected that so comprehensive a form as this is would, in the majority of cases, be filled up satisfactorily; but I am confident that the adoption of some such form of death registration would be the means of accumulating a mass of valuable medical statistics for the purposes of public hygiene."

The Lancet for January 1st, 1870, contains the following abstract of a paper by Pliny Earle Chase, read before the American Philsophical Society:—

"Mr. Chase shows that, notwithstanding the increased juvenile mortality, the Philadelphia life-tables indicate a possible life in Philadelphia of 114 years, a probable life of 33·44, and an expectation life of 35·09 years. He means by the term '*possible*' life, the limit sometimes obtained in a given locality; by the '*probable*' life, the age the probability of living beyond which is as great as that of dying before the age is attained; and by the '*expectation*' life he defines the average which will be attained by all who are born. In sixty-two years the average

mortality was 1 in 47·836, the coloured mortality in the same period being 1 in 27·763. The ratio of still births to total births was 4·3 per cent., and to total deaths 5·8 per cent. The ratio of living births to population was 2·8 per cent., and of deaths to births 74·5. The average natural increase was 3·3, and the increase by emigration 2·6 per cent. The main age at death was 23·57 years, and the main age of persons living was 24·29.

"But the most interesting facts in Mr. Chase's tables are those which show how the simple mode of life of a Quaker community compares with the life of a more active, or, rather, more luxurious people. He analyses the life-tables of the two communities of Philadelphia, dividing them into 'Friends' and 'Philadelphia,' and finds, as his results, that the Friends at the age of twelve years have a maximum vitality of 20·49 per cent. *over* their neighbours; that from twenty to sixty years of age they have a proportionate mortality of 23·37 *under* their neighbours; that their expectation of life is 24·62 per cent. higher, their probable life 43·78 per cent. more valuable, and their proportionate mortality at birth 44·70 lower than the mortality of their neighbours.

"The Quakers of Philadelphia approach thus towards the Jewish race, in respect of vitality, in which they are, probably, exceptional to all other Christian communities. The lesson brings us back in thought to those peoples of whom the student's classical and great master speaks: 'Plerumque tamen eam bonam contigisse ob bonos mores, *quos neque desidia, neque luxuria vitiarant.*'"

VACCINATION.

For several months past a single case of small-pox has not occurred in Ireland, and the loathsome disease may now be regarded as quite extinct in this country. One case did indeed occur in Dublin, but it was that of a sailor who came into port whilst suffering from the disease. The anti-vaccination agitation continues in England, but it is evidently on the decline.

The concluding paragraph of the annual report of the medical officers of the London Small-pox and Vaccination Hospital states that:—

"Considering, however, the prejudices which exist against vaccination, and the efforts which are now being made to foster them, it seems a duty incumbent on us, the medical officers of the small-pox hospital, placed as we are in *the* position the best adapted of any in this kingdom to test the prophylactic value of vaccination, to state emphatically that our confidence in vaccination is undiminished, and that each succeeding year supplies us with additional proof of its marvellous power in

preventing, modifying, and ameliorating small-pox, and this without any proof of those injurious effects, either proximate or remote, about which so much has recently been said."

ADULTERATION OF FOOD AMENDMENT ACT.

The Adulteration of Food Prevention Act, passed in 1860, has to a great extent proved a dead letter. The Act enables the corporations of boroughs, the commissioners of towns, and the magistrates assembled at quarter sessions, to appoint properly qualified persons to act as public food analysts. Up to the present, however, not more than half a dozen public analysts have been appointed in the United Kingdom. Even in towns where such officers exist, the general public do not, to any important extent, avail themselves of their services. The fees which public analysts are empowered to charge for the analysis of articles of food are 10s. as a *maximum*, and 2s. 6d. as a *minimum*. Of course it was presumed by the framers of the Act that public analysts would receive a fixed salary, which is actually the case in London, Dublin, and Birmingham. In Dublin the Corporation have fixed 2s. 6d. as the fee to be paid for the performance of an analysis of food or drink. About fifty analyses of food were made last year for the general public, but, with one or two exceptions, the persons for whom the examinations of adulterated food were made did not prosecute the vendors of the articles. The Lord Mayor's officers purchased a large number of samples of food, and the vendors of those found to be adulterated were prosecuted, and were fined.

Mr. Muntz has brought a bill into the House of Commons, for the purpose of amending the Food Adulteration Act of 1860, and for the extension of its provisions to drugs. It will, if adopted in its present form, be a stringent act. The fee for each analysis is fixed at a *maximum* sum of 2s. 6d. There are to be official collections of samples of food and drugs, and official prosecutions in the cases where those articles are found to be adulterated. The penalty for mixing, or ordering to be mixed, foreign substances with food or drugs, is to be a fine of £50; and on a second conviction of the same offender, he may be sent to prison for six months with hard labour.

The bill should render the appointment of public analysts compulsory on the part of local governing bodies, and it should not be indispensable that the officers must necessarily reside within the districts to which they are appointed. Let us take the case of the

County of Donegal, or that of the County of Roscommon, in neither of which there is a town containing 6,000 inhabitants : is it probable that a skilful analytical chemist, provided with a well-appointed laboratory, could be induced to establish himself in such a district, even though he were appointed county analyst at a salary of £50 or £100 a year ? If chemists residing in the larger towns were appointed public analysts for rural districts, samples of food and drugs could easily be sent to them for analysis by means of the " sample post " recently established by the postal authorities. Townships should have the power of appointing analysts. For example, the Town Commissioners of Kingstown, with its population of nearly 20,000, could not legally appoint a food analyst. This defect in the Act of 1860 ought to be remedied by the measure now before Parliament.

INFLUENCE OF STOVES UPON HEALTH.

The results of the experiments of General Morin, H. St. Claire Deville, Froost, Canet, and other chemists and physicians, appear to show very conclusively that iron stoves, by permitting the passage of the products of combustion through their walls, exercise an injurious influence upon the public health. The late Professor Graham proved that carbonic oxide gas was capable of passing through solid iron, provided the metal was heated to redness. As the front and sides of stoves are liable to become red hot, a large quantity of carbonic oxide generally escapes from the stove into the atmosphere of the apartment. The denser the fuel is, the greater the quantity of carbonic oxide formed during its combustion. Anthracite (in Ireland popularly termed Kilkenny coal), or flameless coal, and coke, produce more carbonic oxide than the bituminous, or flaming coal, and the latter develops more carbonic oxide than turf (peat) and wood. We may regard wood and peat as the fuels which, during their combustion, exercise the least injurious effect upon health. Dr. Derby, of Boston, Lecturer on Hygiene in Harvard University, United States, in a little work on " The Influence of Anthracite Fires upon Health," published late in 1868, states that the anthracite stoves used in America exercise a very prejudicial influence upon health. He recommends the following rules to be observed in burning hard fuel in cast-iron stoves:—

1st. To use fire-brick or soapstone (silicate of magnesia) in contact with the fire, and never to allow the fire-pot to get red hot.

2nd. To have perfect castings, and as few joints as possible; and these joints should be horizontal, and not vertical.

3rd. Never to cut off the supply of air to the fire, and never to check the free escape of the products of combustion to the chimney by dampers, or by cooling the fire by the admission of air between the furnace and the chimney.

4th. To so burn the coal that, under all possible circumstances, a pressure of air from without inwards may be exerted upon the fire-pot radiators and smoke-pipes.

In the number of *Les Mondes* for January 20th, 1870, Dr. Sacc asserts that cast-iron stoves, even if they are allowed to become red hot, are not injurious to health, provided that there is proper ventilation, and a good draught into the chimney—or, rather, into the pipe leading from the stove to the chimney. According to Sacc's experiments, cast-iron stoves are only injurious to health when rendered so by imperfect draught—a defect, however, which applies equally to all kinds of stoves and fireplaces.

INTENSITY OF LIGHT EVOLVED BY COAL GAS UNDER DIFFERENT CONDITIONS.

Mr. B. Silliman has published a very interesting paper on "The Relation between the Intensity of Light produced by the Combustion of Illuminating Gas, and the Volume of Gas Consumed," in *The American Journal of Science and Art* for January, 1870. Mr. Silliman shows, from actual experiment, that the same quantity of gas evolves, by its combustion in large burners, more light than when it is consumed in smaller ones. When a moderate and equally diffused light is required over a large space, it may be expedient to use a large number of small burners; but when a *maximum* intensity of light is desired, burners of ample flow are the best and most economical that can be employed. This subject is of interest in a hygienic point of view, for the less gas burned in our apartments the purer will be the atmosphere. It would be most desirable if the products of the combustion of the gas consumed in lighting theatres and other public places, were prevented from mixing with the air inspired by the audience. Gas might be burned in enclosed spaces, provided with glass sides to allow the light to pass through, and illuminate the apartment, and with a flue through which the products of combustion might be transmitted to the open air.

PART IV.

MEDICAL MISCELLANY.

Reports, Translations, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

Dr. Banks, President.

On the Employment of Blisters in the Bronchitis of Fever. By Henry Kennedy, A.B., M.B.; one of the Physicians of the Cork-street Hospital, Dublin.

That medical scepticism exists widely at the present day few will be disposed to deny. But all familiar with the history of medicine are aware that more or less of this has prevailed at all times: and, indeed, for myself, I have the strongest conviction that it will continue. There would appear to be some minds incapable of arriving at any definite conclusion—on such data, at least, as medicine affords, and can only afford. Still, as those minds constitute but a very small proportion of the profession, medicine, as a science, has received no injury from them; and so it continues to be a progressive, and, as I believe, an improving art. The work of the late Sir John Forbes was amongst the first of recent times to call in question the powers of our art over diseases. Before it, however, it will be recollected, that what I may call the blood-letting controversy had arisen—the most important question which has arisen in our own times. Since this, some of our most valued and long-tried drugs, such as calomel and tartar-emetic, have been denounced in no measured terms. The use of wine in disease has been ignored; and it is scarcely a year since it was gravely put forward in the leading

medical society of London that acute rheumatic fever was best treated by the administration of that potent agent, mint water. It will be recollected that I brought this question before the association at the time, and it had the effect of bringing out the almost unanimous opinion of the profession in Dublin that the alkaline treatment of the disease led, by much, to the best results. Within the last year, too, we have had the question raised in England, whether vaccination be a blessing or the contrary; and you are all aware what a controversy is going on at the present minute about it. Lastly, I hold in my hand a recent number of the periodical known as *The Practitioner*, which contains a paper by a French physician. The paper, too, has the strong sanction in its favour of the editor, and is headed, "On the Dangers of Blistering, especially in Pneumonia."

Now, in making these remarks I have had two objects in view—the first being to protest, in the strongest terms, against one and all of these medical scepticisms. I believe that medicines, like calomel, tartar-emetic, and alkalies, will continue to be used; that wine will not lose its place as a remedial agent; that vaccination will still be adopted; and that the theories advanced against blisters—and I must say they are all theories—or their dangers, as the Frenchman has it, will not prevent their being used as generally as they ever were. And this leads on, in the second place, to the more immediate object of this paper, which is, to make a few remarks on the value of blisters; though they have of late been so much decried.

The principle of counter-irritation, in its many forms, is, I need scarcely say, one of great extent and no little importance; so much so, indeed, that on the present occasion I must limit myself to a very small portion, if I may so speak, of the subject; and that is to the value of blisters, chiefly in one of the complications which arise so often in the progress of our fevers—I mean those of the chest.

In the treatment of fever it must be allowed that as yet we have little or no power over the disease *per se*. I am sanguine, however, the time will come when this will be accomplished; and that some antiseptic or alexipharmic, as it used to be called, will be discovered, which will prove to be antagonistic to the poison of the disease.[a] But it is far otherwise as regards the complications which arise, and which, in a very large number of instances, really place the life of the patient in danger, or lead directly to a fatal result. Over these, I presume, there are few who

[a] The medicine I at present use, and have the greatest confidence in, is barm. I am speaking here of spotted typhus. Under its use the spots very soon change from a dark to a red hue; and, what is of more consequence, the mortality has, I believe, been brought to the lowest on record. Having spoken on this particular subject on another occasion, I cannot allude to it further here.

will assert we have no control; even though this statement be in direct opposition to the often-expressed idea that in fever we can only support the strength of the patient, while the disease is passing through its stages, I find it hard to understand such a statement, or how, in making it, the complications of the disease have been so entirely overlooked. For myself, I must say that, as far as I have seen, cases of pure uncomplicated fever which have proved fatal have been very exceptional, whilst, on the other hand, complications arising in the course of the disease, and proving fatal, have been far in a way the general rule. Now, of those which occur the affections of the lungs are probably the most frequent, though, like other complications, very much more common at some periods than others; as, of course, every one familiar with fever is quite aware. And indeed these statements are what might be called truisms, so well are they known; but it was necessary for my purpose to state them here.

Of the affections of the lungs occurring in fever, bronchitis plays the most important part. It is by much the most frequent, for pneumonia is rare in comparison; and it is far the most serious, for it affects life much the most frequently; and, what is never to be overlooked, it is very difficult of cure. Comparing the bronchitis of fever with the pneumonia of fever, the latter is much the more amenable disease, and consequently the less serious. It is, however, far from my intention to say anything here of bronchitis as it arises in fever. It is enough to know that it occurs, and in both, or rather all the types of fever, for there are several, but more particularly in those known as typhus and typhoid; and though the bronchitis in these two assumes a somewhat different aspect, which ought to be recognized, I cannot stop to inquire into it here. When it arises, then how is it to be met? Besides other treatment—including chiefly stimulants, and, of course, wine—blisters, I firmly believe, will be found to be of the greatest value, and that life will often be saved by their proper use. Here I cannot but notice a practice which has of late years crept in, and superseded, in a degree, the use of blisters—I mean turpentine stupes. I do believe no more fatal mistake could be made in grave cases; for I hold that no stuping even approaches in its effects that of blisters, and that precious time will be lost by the former mode of procedure. I do not profess to enter here into the question how blisters act. I leave this to the editor of *The Practitioner*, who, not being able to advance a theory on the point satisfactory to himself, decides against their use. For myself, I prefer to be guided by experience, and the result is that I am diametrically opposed to the opinion of the gentleman of whom I speak. Were we to give up remedies because we cannot explain their mode of action, the treatment of disease would be turned into a farce.

But to render blisters effectual in the bronchitis of fever there are one or two points worthy of note. In the first place, they ought to be large. Obvious as this point is, I have reasons for knowing it is frequently overlooked. Parties forget the great extent of surface involved in most cases, and the blister, when ordered, is not at all commensurate with the extent of the disease; hence failure is too often the result, and even when relief does follow the small blister it does not conquer the disease. Size, then, I take to be a most important element in the use of blisters in those cases; and by size I mean here one that would cover the whole front or back of the chest. Of the two surfaces I believe the latter to be the best; but we must often have recourse to both to save our patient.

Probably the meeting will allow me to introduce here a few cases given in the briefest way, in which blisters used as I have advised were the chief means of saving life. Cases given as examples may, in this way, impress more forcibly the points I wish to convey.

Case I.—Crickkard, a girl aged twenty-two, admitted to hospital labouring under fever of a low type. In the course of three days she became spotted, or rather blotches of a very irregular shape appeared; some much larger than ordinary petecchiæ. In this state, and when now about ten days ill, bronchitis came on, and increased to the most serious degree, the distress being very great for more than a week; the pulse reached 140, and the hands and lips were livid; and, what showed more than anything else the danger, was the fact that the brain, which had been heavy and stupid, became clear and natural, and alive to the danger which existed. By powerful blistering this case came round; nor did she get more at any time than ten ounces of wine in the twenty-four hours.

Case II.—Brophy, a girl of twenty, sent in by Dr. Mollan; passed through a very severe attack of enteric fever, during which a good deal of bronchitis existed. After improving a little from this state she relapsed, and the bronchitis became the most serious possible. The pulse rose to 140; the lips got livid, and the distress for three nights was extreme. During this period she was powerfully blistered; and it was particularly noticed, that while the general treatment was the same, the relief obtained and expressed by the patient was most marked; and she finally, though after a very severe struggle, recovered.

Case III.—Martin, a boy of twelve years of age, admitted to hospital labouring under typhoid fever, well marked. His chest, too, was early engaged, his lips livid, and his distress from dyspnea very great. The pulse was very rapid, and there was constant raving. In this state his life was in the balance for many days. Finally, by relays, as I may call

them, of blisters, the chest symptoms gave way, and the patient recovered.

Case IV.—O'Neill, a girl of sixteen, passed through a severe attack of fever, with spots, during which her life was in the greatest danger from an attack of bronchitis. Her pulse became excessively rapid. Under powerful blistering this case also came round; and, I would again observe, without any change in the general treatment.

In this way a large number of cases might be given. But such is quite unnecessary; they would be similar to those described, and only agree in the point of all being most urgent from the lungs being engaged. It is particularly to be observed, however, that even when they proved fatal, as they often did, there was the clearest evidence that, in a degree, they had been of use; for it was quite obvious to myself, as shown by the lessened labours of the respiration, and the patients themselves often expressed relief, even though the case ended fatally. It is, of course, well known that our poor frequently will not come into hospital till the urgency of the symptoms forces them; and in no case is this more common than in the very complication of which I now speak. And, in fact, too many of them are admitted only to die. This leads on to a second point in connexion with my subject, which I must notice, which is the great value of blistering quickly. I am satisfied that this is an important principle to keep in mind, and that it may save life, where ordinary blistering, to even the same extent, will fail. It seems to give a kind of revulsion or shock to the system often beneficial; and I cannot help repeating, that the poor patients themselves have often at the time expressed relief from it. Hence in these cases I am in the habit of having the chest at once rubbed freely with the acetum cantharides, and this immediately followed by a blister. By this mode a discharge is rapidly induced, and it is the discharge which benefits the patient. I shall just give one case bearing on the point.

Case V.—Mangan, a man of fifty-five years of age, admitted February, 1863, in an advanced stage of typhus fever, being much spotted. He had all the appearance of a man about to die. He had constant raving, and was passing under him; but by far the most serious symptom about him was a very extensive bronchitis engaging the minute tubes in both lungs. In this state, besides stimulants, he was blistered in the way described; the entire chest being blistered in the course of twelve hours. I must say I was as much surprised as I was pleased to find this man a shade better the next day; and it is enough for my purpose to add that after a fearful struggle he came round.

There still remains one point to be noticed in connexon with my

subject. It is by no means uncommon to meet cases where, though life is not in immediate danger, still the patient does not recover. More or less fever remains, as well as bronchitis, and the pulse keeps much above the ordinary standard. In these cases marked benefit has arisen from converting a blistered surface into a kind of temporary issue. This has been done by the use of a very weak tartar-emetic ointment, which, however, requires caution. Twice ugly marks were left after the parts were healed. A safer plan is the use of one or, occasionally, two Albespiere's papers. It has been, in cases of enteric fever especially, that this mode has been found useful, where, as is well known, signs of hectic and even phthisis are very apt to arise. I cannot doubt I have seen life saved in this way ; and were it necessary a large number of cases could be given bearing directly on this point. But these would exceed my present limits. It is right to add, that the blisters are only allowed to remain on four hours ; that is, speaking generally of them ; and when it has chanced that they have not risen well they have been re-applied over the same surface. It is also well to state, that though they have been used in very many cases of fever of the lowest type, as shown by the presence of a large crop of dark petecchiæ, no casualty worthy the name has occurred, nor have I ever seen the surface slough. In conclusion, then, I would just re-state the three principles, in the use of blisters, brought before you this evening, believing them to have a special practical bearing on the successful treatment of the bronchitis of fever :—

1. That the blisters ought to be much larger than those commonly used.

2. That in many instances rapid blistering is followed by the most decisive results.

3. That the conversion of a blistered surface into a temporary issue is often very beneficial.

P.S.—In *The Practitio er* for March, 1870, I observe the editor has again brought forward the subject of counter-irritation. The entire paper is occupied with the theory of the matter. But in the course of it he speaks of the danger to life, not only from blisters but even from turpentine stupes. There must surely be some error or exaggeration in such a statement.

Polyuria in some Forms of Chronic Renal Disease, with Remarks on the Physiology of the Kidney. By J. Magee Finny, M.B., Dub.; F.K. & Q.C.P.I., &c. ; Demonstrator of Anatomy in the School of Physic, Ireland.

Having for some time past had my attention and study directed more particularly to the consideration of diseases of the kidneys, the symptom of polyuria in cases of chronic renal disease attracted my notice as

being, not only a very remarkable symptom, but one difficult of explanation. Many of these cases were proved, by *post mortem* examination, to be examples of the small contracted cirrhosed kidney, with its cortical portion so shrivelled and degenerated that the wonder was not so much that a large quantity of urine was passed per diem, but that the possessor of such altered and impaired organs could have survived so long.

For the explanation of this secretion of urine, either equal to, or in excess of, the normal average per diem in kidneys so diseased, I have referred to most of the books written on the subject within my reach, including the standard works of the modern times, but failed, in many instances, to obtain any information whatever, and in those in which this symptom was treated of, to derive much benefit, or to clear up the difficulty I experienced, a difficulty I feel confident has been shared in by many others. This brought me to examine for myself more closely the physiology, pathology, and histology of these important depurating organs as at present known, and the result of my study has been to assure me—and in this assurance I am not alone—that we require much correction and revision in our ideas on this subject, and that the theory so universally accepted and taught of the mode of secretion of the watery constituent of the urine needs to be, if not modified or entirely altered, carefully investigated and examined before being thoroughly accepted, since its tendency is, I believe, to retard us in the right comprehension of those pathological changes so often presented in chronic Bright's disease.

Since the general symptoms of the cirrhosed and the amyloid kidneys—the two forms in which polyuria is observable—are so well known and clearly defined, I shall not delay to dilate further upon them than to lay stress upon the almost invariable increase of urine which obtains at some period of the diseases alluded to. Neither shall I enlarge upon their morbid appearances, but call attention to the remarkable fact, that in both these kidneys the cortical portion is that which is chiefly and primarily diseased: in the one, due to the amyloid substance engaging at first the Malpighian capillaries (as definitely determined by Dr. Grainger Stewart[a]), and from that invading the vascular plexus and the tubules composing the cortex; in the other, due to atrophic degeneration, progressing in some cases to such an extent that the bases of the pyramids reach to the capsule; and so in each, though differently attained, the Malpighian bodies to a great extent—those very structures to whose agency we are taught to look for the elimination of the watery constituent of the urine—and the tubuli uriniferi in the ends of which these bodies lie, are rendered impervious and useless.

Polyuria as a symptom, and that a prominent one, of the contracted

[a] Bright's Disease, p. 67. 1868.

kidney, does not seem to have attracted the attention, nor received the consideration it deserves at the hands of the many writers on this subject. Indeed, in searching through the multitude of literature with which kidney disease is covered, I was much struck by the rarity that any notice whatever was taken of it as a symptom by no means easy to explain.

Dr. George Johnson, one of the few whose work alludes to it, suggests the probability, "that when the tubes continue to be nourished after the removal of their normal epithelium, this is associated with some modified secretion, or growth within them. I have sometimes thought," he writes,[a] "that the abundant secretion of urine of low density, which is so commonly associated with this form of renal disease, might be in part explained by supposing that some of the tubes when denuded in the manner described, having lost their normal epithelium, and with it the power of secreting the solids of the urine, may yet continue to pour out a watery secretion, which would dilute the urine and diminish the density. Another explanation of the abundant flow of watery urine is one which," he continues, "I have before suggested (when speaking of this symptom occurring towards the convalescence of acute nephritis after scarlet fever), viz., that the particles of epithelium which are thrown into the tubes stimulate the Malpighian corpuscles and excite them to pour out a current of liquid, the purpose being to wash away the debris and clear the tubes."[b]

The second whom I shall quote from, and whose opinion must carry considerable weight, is Dr. Roberts.[c] In his valuable and able work on urinary and renal diseases, alluding to this point, he writes—"It is a curious circumstance, that in several organic diseases of the kidney in which the renal substance is gradually destroyed (atrophic, Bright's disease, cystic degeneration, double hydronephrosis), the volume of the urine is sometimes increased though the solid matters are diminished. This appears to be an attempt on the part of nature of a compensating character, to maintain by *excessive transudation of water* the depurating functions of the kidneys under failing anatomical conditions."

The last I shall refer to is Dr. Grainger Stewart, who has so recently written on the subject. In answer to the inquiry, "How does it happen that while much of the kidney is atrophied, a natural or even excessive amount of fluid transudes?" he suggests,[d] "It may, perhaps, be explained by the fact that increased blood pressure is exerted on the Malpighian bodies which are still in action; but such an explanation is unsatisfactory, inasmuch as though it might account for the urine being not diminished, it cannot account for the actual increase. There may

[a] Diseases of the Kidneys. 1852, p. 214.
[b] *Ibid.*, p. 114.
[c] Urinary and Renal Diseases. William Roberts, M.D. 1865, p. 21.
[d] *Op. cit.*, p. 127.

be some change," he continues, "in the capillary walls whereby excessive transudation may occur; but such a change has not yet been discovered. It is not unlikely that in many cases the amount of urine is regulated by the amount of fluid consumed, but this cannot be accepted as the sole explanation, seeing that in one case experiments were carefully conducted from which it appeared that diminution of the supply of fluid ingested did not correspondingly influence the secretion of the urine. Perhaps," he goes on to say, "the true cause may one day be found in some abnormal condition of the blood, whereby its watery parts more readily permeate the capillary membrane." Again, in his remarks on the albuminoid kidney, Dr. Stewart takes the same view as to the cause of the polyuria. "This symptom," he writes, "appears referable to the degenerated state of the vessels. It may be that the degeneration of the muscular fibres of the small arteries leads to paralysis and dilatation of these vessels; but certainly we are entitled to assume, that the degenerated walls, although thickened, permit undue transudation of their contents."[a]

In seeking a solution of this question of polyuria, I conceive we are looking in the wrong direction for the light when the Malpighian bodies are viewed as the sole, or even as the chief, factors in the secretion of the watery constituent of the urine, although in making this statement I know I am at issue with the most distinguished and most talented authorities on the subject. There seems to me to be no occasion for accepting, as explanatory of the phenomenon, the stimulating action of dead epithelium on the Malpighian bodies, or the altered functions of living epithelium (as Johnson supposed); nor the theory of the compensatory secretion of water for solids (according to Roberts); nor, lastly, the exalted action of the Malpighian bodies and increased permeability of their walls, when the vessels forming these bodies are thickened to two or three times their normal size, or degenerated into an amyloid substance, which, I think, we have no reason to assume with Dr. Stewart is more permeable than the tissue composing the wall of healthy blood-vessels. A simpler explanation must, I feel confident, exist, and warranted by the advanced teachings of anatomy and physiology, such an one I would venture to suggest; while I am sure that, not one, but several causes combined, are at work in the production of this symptom of chronic renal disease. I believe the difficulty (and that there is a difficulty in accounting for this diuresis the explanations detailed above, by their variety and, as I read them, their want of sound worth plainly testify) is due in a great measure to our looking upon a small part of the kidney as though it were the whole kidney, and by our attributing to this part offices and functions shared in common by the whole.

[a] *Ibid.*, p. 90.

There can be no doubt that the obstacles in the way of the investigation of renal disease are many and great, both for experimental and microscopical study, and ought to be borne in mind in observing the inaptitude, and I may say the sluggishness, with which any new point is examined into by most men; and this I say, without, in the smallest degree, advocating that questionable thirst for novelty and claptrap so prevalent in the literature of the present day.

Moreover, as Dr. Roberts justly remarks,[a] the diversity of opinion and discrepancies between the descriptions of the various authorities of the morbid anatomy and the structural changes produced in the kidney in Bright's disease can only arise from, on the one hand, the uncertain state our knowledge is in with respect to the normal anatomy and physiology of the organ; and on the other, to attributing to diseased action (as Dickenson[b] lays stress upon) minute changes in the tube contents and in the epithelial cells themselves, which may all be found as occasional phenomena in kidneys in every other respect both functionally and anatomically healthy.

The researches within the last few years of Henle, Ludwig, Virchow, Schweigger-Seidel, and other German inquirers, and still more recently, of Beale, Dickenson, and Southey, make it abundantly evident that the description of the anatomy, as given by Mr. Bowman, and copied into every text book since then, must not be accepted entirely without modification or addition, and show that the course of the uriniferous tubules or of the blood-vessels is very far from being so simple as has usually been supposed.

In order, therefore, to place us in a right position to understand the physiology of the kidney with special regard to the water secretion, and, therefore, to read aright the changes produced in this organ by the diseases under consideration, I shall briefly refer to those discoveries in the tubular and vascular arrangements which may be considered as now generally recognized facts, and which bear directly upon our subject.

And here I would gratefully acknowledge the assistance a paper by Dr. R. Southey, in "St. Bartholomew's Reports," has given me, which I recently came upon, some time after I commenced this communication.[c]

The tubuli uriniferi, which by Henle, in 1862, were considered to consist of two distinct sets—some *open*, ending in a papilla; others *closed*, including the Malpighian bodies, but having no communication with the rest—have been proved, and, I may say, now positively demonstrated by Ludwig, Beale, and others, to be one and the same tube, only under different conditions and in different situations, confirming so far

[a] *Op. cit.*, p. 297. Note.

[b] Path. and Treat. of Albuminuria, 1868, p. 2.

[c] Minute Structure of the Human Kidney. St. Barthol. Reports, Vol. i., 1865, and Vol. ii., 1866.

the views of Bowman. Following the course of the uriniferous tube, beginning in the Malpighian body, we find that it immediately falls into a knot of windings and convolutions; emerging from these, it bends downward towards the pyramids, and following a straight course descends into one of these to a variable depth; it then turns and reascends to the cortical part; here, after a second and similar series of convolutions, it finally bends down again straight into the pyramidal portion, when it unites with other straight canals; and at last, greatly enlarged, it reaches the open mouth of a papilla.

The vascular supply to the kidney, in its component parts, is still more important clearly to understand, though not less complex and difficult. The vasa recta are those vessels about which the greatest interest lies, and with which I shall confine myself. The opinion of Bowman, that they arose from the efferent vessels alone has not been confirmed altogether by the subsequent investigators. For while some vasa recta, no doubt, and that in no very small quantities, come from the efferent vessels, there seems to be a very general conviction that, on the other hand, the arteriolæ rectæ spring direct from the renal artery without any intervention of the glomerulus in the Malpighian body. Virchow[a] originally made this discovery of a distinctly different origin for the vasa recta, and of a free anastamosis between the two systems at the base of the pyramidal portion. Dr. Beale confirmed Virchow's statement in 1856, and in his most comprehensive work, republished last year,[b] he expresses himself convinced of the correctness of this conclusion, after having fully re-examined the subject. Similar views are maintained by Ludwig, Hyrtl, Arnold, Dickenson, and Southey. The latter considers that in a healthy kidney the vasa recta spring mainly from the efferent vessels (agreeing thus with Bowman's and Berres' views), and also, in a smaller number, from direct capillarization; but that in a diseased state, more particularly when the vessels forming the Malpighian bodies are obstructed, and to this particularly I call attention, the vasa recta, which spring from the renal arteries, become very large and distinct. For example: in the amyloid kidney (from the examination of which Virchow has made his observations, and drawn conclusions somewhat, perhaps, too dogmatic), and in a similar specimen in Dr. Southey's possession, where a tubercular growth obstructs the vas ascendens to the lobule, the vasa recta are brought to view distinct and enlarged. That a similar change takes place in the cirrhosed kidney when the adventitious tissue compresses the Malpighian bodies is most probable, although I regret my inability to speak positively on the subject, notwithstanding having made several microscopic examinations with reference to this point. From these facts, then, it may be legitimate to assume that the medullary portion of the kidney, consisting as it does

[a] Archiv. f. Path. Anat. and Phys., Vol. xii., p. 310.
[b] Kidney Dis. Urin. Deposits. 3rd Edition, 1870.

of tubuli uriniferi, both looped and straight, will receive through the vasa recta a considerably greater supply of blood in all instances when the cortical intertubular region is impaired and the Malpighian capillaries obliterated, than in a state of health, or in those morbid states where the disease is principally confined to the epithelial lining of the tubes, as, for example, in acute or chronic desquamative nephritis.

There is, I suppose, no question on the mind of any as to the convoluted tubes being the chief *locus* of the secretions of the mass of the solid constituents and pigment of the urine, although Ludwig, Kölliker, and more recently Dr. Isaacs, of New York,[a] have claimed for the Malpighian bodies the office of the secretion of urea. There is not, however, much thought usually bestowed upon the medullary portion; and yet, when we bear in mind the close and intimate relation of the blood-vessels to the tubules in the pyramids, many of these having direct origin in the renal artery, the tubules themselves taking the peculiar course described above, and remember that the epithelium of the looped tubes is similar to that of the convoluted, the question is forced upon us, What is the function of this extensive tube territory; what part does it play in the secretion of the urine? These tubes must be something more than mere sewers or pipes laid to carry off the solids washed down, as some suppose, by the water charged with oxygen transuding from the Malpighian bodies—must have some higher function than calyces, infundibula, or ureters. I claim not for this portion of the kidney *as* high or *as* important offices as the cortex, nor fix upon it any peculiar property unshared by the rest, but I do claim for it higher qualities than ordinarily accorded it, and consider it a most important part of the organ by which compensatory work may be performed, either temporarily or permanently, according as the cortex be temporarily or permanently disabled, and upon the healthy condition of which, rather than of the cortex, the life of the individual seems more intimately to depend.

I believe the secretion of the urine, more particularly its fluid portion, takes place as much in the medullary region as the cortical, and is in reality the product of the active agency of the epithelial cells lining the whole uriniferous tubules.

The different character of the epithelium in the convoluted and straight tubes may be urged as an objection. But I do not consider it a great difficulty; for, though the epithelium lining the tubes opening on the papilla and those in the cortex are different, yet all through the pyramids the epithelium presents more of a transitional character, and is decidedly of a spheroidal type, verging to, we may say, and yet distinct from, the squamous character which it becomes near the papilla.

Further, I would say that although we constantly look to the spheroidal

[a] Trans. of New York Academy, Vol. i.

epithelium as the active agents of secretion, we have no right altogether to deny similar properties, though limited, perhaps, to epithelium somewhat less glandular, for, as Bowman observes, it is extremely hard to draw the line of demarcation between secreting epithelium, and that merely protective.

The theory of the kidney being but a filter of either solids or fluids, I, for my part, repudiate, notwithstanding that such views are to be found in some standard works on physiology and on diseases of the kidney. On the contrary, I state it as my belief that the water is as much a secretion as the solids, and for its secretion we must look to the epithelial investment of the tubes.

Against this view it may be urged, that the water of the urine is eliminated in a manner analogous to the filtration or transudation of water through the skin. Although, as Professor Huxley[a] puts it, perspiration would still continue to take place, to some extent, even in the absence of sweat-glands, as a process similar to what occurs in the lungs, the comparison will not hold good, I believe, when applied to the kidneys, since in them we lack the differing temperature and the differing specific gravity of the fluid within the capillary vessels, and of the surrounding medium whose hygrometric conditions are hourly undergoing variations, which obtain in the case of the skin.

Further, I would say that *osmosis* ought not to be applied to the elimination of the water at the Malpighian bodies, for that process, as I understand it, requires two fluids miscible, but of different specific gravity, to be separated by a thin septum of animal membrane gas or fluid, through which they pass until the specific gravity is equalized at either side. Now, in the kidney, although I believe osmosis may, and most probably does, take place from the vessels, forming the plexus around the tubes of the cones, into the tubules, at the Malpighian bodies at least, since the necessary conditions are absent, an exosmosis from the blood can hardly take place.

Again, if in the kidney, at the Malpighian bodies, the water of the urine be merely a filtration from the blood, as is universally taught, it is the only instance in the body where such takes place. I know of no other example where, under high pressure from behind, the liquor sanguinis can be drained of its water without some of the plasma passing along with it. Should such a condition of arterial pressure or venous obstruction be morbidly or accidentally set up in any part of the body, even in parts where the blood-vessels are few and large, we know albumen is certain to be found in the effused serum.

Let us, however, for a moment suppose such to be the normal course of things, that 50 oz. of water are naturally drained off per diem from

[a] Elementary Lesson in Physiology. Huxley, p. 129.

the blood, and are we not compelled to ask, what is it that gives this membrane, the capillary wall in the Malpighian body, so thin and delicate, the power of retaining some matters of the plasma and allowing only water to permeate its walls. But supposing, again, such power is really given the capillary membrane—although totally inexplicable to me—while the tissues of the kidney are normal and unaltered by disease, how is it possible that water can still permeate their walls in even a greater quantity than before, when these vessels are obliterated and destroyed, or the tubes into which they are supposed to pour their fluids are impermeable. Are not these great difficulties, and does not the filtration theory entirely fail to meet them?

If we turn our consideration now, for a few moments, to the lessons derivable from analogy, we will find, I believe, additional reasons for rejecting the theory that to the Malpighian bodies belong the special function of separating the watery constituent of the urine, inasmuch as we shall see that in the kidney, at our very hands, so to speak, lie the very same structure capable of secreting water which exist in other glands, and which all bear the type of unity so visibly stamped upon them by the hand of the Great Creator.

The amount of healthy urine secreted in twenty-four hours, averaging 40–50 oz., is not of such magnitude as to raise, in my mind, any difficulty in referring its source to the epithelium lining the tubules convoluted, looped, and straight. Dr. G. Johnson, however, considers the volume of water entering into the urine as something very difficult to account for, and, in fact, as something very peculiar to these organs. He says,[a] "The secretion of the kidneys differs from that of all other glands in the abundance of water it contains; and," he goes on to say in explanation, "that this water is eliminated by the Malpighian bodies, the singular position and all the details of the structure of these bodies, render in the highest degree credible."

Now, on referring to *Carpenter's Physiology* (6th Edit.), I find the amount of fluid secreted by the liver was estimated by Nasse and Platner to be, for a man of eleven stone weight ℥40 f., or 2½ lbs.; the same amount is also given by Dr. Flint,[b] while Bidder and Schmidt give a still higher estimate, 56 oz. or 3½ lbs. Again, saliva, which varies under the varying wants of the body and the supply of fluid ingested, may be set down at 2 to 3 pints in 24 hours.

Again, with regard to the cutaneous secretion and transpiration, the amount of fluid, according to Funke, over the whole body, fluctuating from 2 oz. to 29 oz. per hour, is on an average equal to 2¾ lbs. in the 24 hours. The last other secretion I would refer to is that of the stomach, which is really astonishing, amounting in a healthy man to 10 or 20

[a] *Op. cit.*, p. 59.

[b] Phys. of Man. Dr. Austin Flint, jun., 1870.

pints in 24 hours, while under favourable circumstances its maximum in one hour may not be less than 6 or 8 pints.[a]

These examples cited, which although not all glands of like functions or excrementitious glands, nevertheless present, with the exception of the Malpighian bodies, similar anatomical arrangements as the kidney, are fully sufficient to show that the amount of normal urine is not anything very peculiar; nor need it be considered a startling presumption to refer its secretion to the epithelium of the tubes, especially when we bear in mind the size and complexity of the kidneys, whose tubular surface has been estimated by Valentin at *six times* the whole amount of cutaneous secreting tissue, and reflect that the absolute quantity of blood traversing the healthy kidneys amounts, according to Brown-Sequard, to 2,000 lbs. per diem, or according to Professor Haughton, 566 pints.[b]

What is the arrangement of these glandular organs with regard to their vascular supply and epithelial lining? In the stomach, the salivary and the sweat-glands is not the essential parts exactly as in the kidney, namely, a tube lined by epithelium, glandular, columnar, and squamous, and having outside a network of capillaries in close and intimate relation. In none of the secretions alluded to is there any difficulty in accounting for the water which enters so largely, as we have seen, into them. We refer it, and rightly so, I believe, to nothing else than the vital electivity of the epithelium influenced by the amount of blood supply under nervous control. As, then, in the kidneys we find, I assert, the same structures in these glands, there seems no need to refer the elimination of the water to anything else than to them; nor does it necessarily follow, that because the Malpighian bodies present the peculiar features they do as to relation and structure, they are directly connected with this secretion; for if, with Prout, we accept the assimilation of water during the processes of assimilation, like the processes of assimilation themselves, to be a *vital* act,[c] I see nothing inconsistent with common sense and physiological data to ascribe the elimination of the water to a similar vital act on the part of the epithelium of the kidney.

That the Malpighian bodies have some peculiar functions which are of vital importance to the integrity of the kidney as a whole there can be doubt, but what there exact office may be it is not easy to pronounce dogmatically; but that it is not that of a water-filter seems still further confirmed by the very fact of their being found in considerable numbers in the urinary apparatus of birds and reptiles, into whose urine water, as a constituent, hardly enters at all. Kölliker, and more recently Dr. Isaacs, of New York, have maintained that urea is secreted by these bodies, but

[a] Diseases of the Stomach, Dr. Brinton, 1859, p. 32.

[b] Haughton on the Phenomena of Diabetes Mellitus, Dub. Q. Journal, Vol. xxxi., p. 377, 1861.

[c] Prout on Stomach and Urinary Diseases, 1840.

I see no reasonable grounds for supporting such a theory. More recently still, Dr. R. Southey has propounded a theory of their being *blood regulators*, and this theory I am most inclined to adopt, partly for want of a better, and partly because, on thinking it over, it strikes me as one having much of probability to recommend it. As his views may not be familiar to all, I shall quote his own words.[a] "All the blood," writes Dr. Southey, "passing through the kidneys makes its *premier début* on those little stages, but explains nothing of the *rôle* played out by them. No; there is something much more important effected at these blood-gates, which no engineer would have overlooked. Our explanations hitherto have been too depreciatory of Nature's handiwork. Through blindness we have ignored, perhaps, the most beautiful example of constructive design in the Creator's mind, which is presented in man his masterwork. Here is the sluicegate which commands supply; the balance wheel which regulates speed; the governor which prevents undue pressure; an escapement adapted to work for scores of years and not be worn out, or thrown out of gear."

In accordance with this theory the Malpighian bodies act as a sort of breakwater to the velocity of the blood, which otherwise passing with unchecked force into the capillaries surrounding the tubules of the cortex would most certainly rupture their thin walls, and hemorrhage into the substance of the kidney and the tubuli uriniferi, would be a phenomenon of constant occurrence.

There are, however, as we have already laid stress upon, other vessels which naturally act as safety valves to the Malpighian bodies, should the permeability of these bodies be temporarily obstructed, but which become enlarged, and to a certain degree constitute the chief channel by which the circulation of the kidney is maintained when these Malpighian bodies are rendered useless, and the vascular plexus beyond them impermeable by disease, namely, the vasa recta. Through these vessels, ramifying as they do among the tubules composing the cones, the blood, unchecked in its velocity on account of the impermeability of the cortex, will flow in a very full stream indeed, and, accordingly, it is most probable that so long as the obstruction to the cortical circulation lasts, and so long as the epithelium lining the tubes of the cones remains healthy, a rapid and large secretion of watery urine will take place in the region.

The loss of a regulating power, due most probably to a dilatation of the blood-vessels, produced (as Parkes remarks[b]) by paralysis of the constrictor nerves, is we know, a pregnant cause of diuresis.

In the state of great fear, in hysteria and other nervous affections, polyuria is well known to occur. Bernard's experiments of division of the sympathetic nerve in the neck, which was followed both by a diabetes

[a] *Op. cit.*, Vol i., p. 185.

[b] Parkes on Urine, 1860, p. 364.

insipidus and a great flow of saliva from the glands on the side operated on,[a] seem to place beyond doubt that the dilated state of the blood-vessels is followed by a watery secretion. Now, I believe that the same permanently dilated state of the vessels of the cones may be, and is mechanically produced when the Malpighian bodies are obliterated; and, accordingly, on the supposition, which I believe is correct, that naturally the tubes of the cones are actively concerned in the secretion of the urine, a rapid and large secretion of water will take place in this medullary region when the cortical portion is diseased or obliterated.

Having thus, to some extent, shown the views of the physiology of the kidney, which, I believe, may fairly be deduced from the more accurate acquaintance with the anatomical relations of the tubules and their vascular supply, and having endeavoured to show that Bowman's views cannot be accepted as to the secretion of the water of the urine, let us see how far the symptom under consideration can be reasonably explained; how the 2, 3, 4, or 5 pints of water can be secreted by organs so imperfect as the small contracted and the amyloid kidney; how it is that under these altered conditions the flow of water exceeds that of health in some stage of the disease.

To the due performance of the functions of any secreting or excreting gland three conditions, at least, must exist, exclusive altogether of nerve power, not that it is not a most powerful agent, but because in the case of the kidney our knowledge of the nervous influence is so slight, that we should have nothing better to rest upon but hypothetical theories. 1st. the blood, from which the fluids and solids of the secretion are to be derived, must be healthy both in quality and quantity; 2nd, it must be brought into close connexion with the elective epithelium of the tubes; 3rd, the epithelium must be itself in a normal state. The latter condition fails in acute tubular nephritis, as well as in the fatty degeneration of the organ; in the one, due to the turgid, too rapidly developed, epithelial cells, and fibrinous exudation into the tubules; in the other, to the epithelium rendered equally useless through fatty degeneration. It is thus easy to understand that in these two diseases the flow of urine should be less than normal, and such, by clinical experience, we learn to be the case.

In cirrhosis of the kidney, on the other hand, whatever epithelium does exist is normal; but the blood brought to the epithelium is considerably altered, and much deteriorated.

The following remarks upon this point have been chiefly compiled from Dr. Christison's statements, quoted by Dr. Johnson,[b] and from Dr. Owen Rees:[c]—

I. The serum is more abundant in proportion to the clot; the buffy

[a] *Ibid*, p. 100. [b] *Op. cit.*, p. 189. [c] Dis. of the Kidney, p. 40.

coat less frequently appears, and when it does, the clot is remarkably small and contracted; the fibrine is usually natural in its proportions, and only occasionally abundant when some local inflammation is set up.

II. Towards the period of the disease, when atrophy and shrinking of the gland substance is manifest, the serum is low in density and albumen is decidedly deficient. This deficiency of albumen in the blood is sometimes so marked, that the serum, when examined, is occasionally found so low in specific gravity as 1018, or even 1015, the standard of health being about 1029–1031. Very important changes are by this loss of albumen produced in the physical characters of the blood, the liquor sanguinis becoming watery, and leading to several secondary evils.

III. The hematosin gradually diminishes as the disease advances, and as length is reduced so much as to form less than one-third of the healthy average, so that, as Dr. Christison justly remarks, there is no chronic disease which so closely approaches hemorrhage in its power of diminishing the red particles of the blood.

Blood, then, of this poor watery character, is that fluid which usually in cirrhosis of the kidneys, circulates through these organs. Brought with greater force than normal to any remaining tubules in the cortical portion owing to the destruction, and of so many Malpighian bodies, brought with even still greater force, and in fuller current to the tubes of the medullary portion, a rapid secretion, we believe, of urine ensues; poor indeed in solids, and of low specific gravity, but in a quantity greater than normal, while its albuminous character may, no doubt, be attributed to some of the blood plasma having transuded the walls of some capillary vessels, whether at a Malpighian body or elsewhere, under the undue blood pressure.

Two other conditions are very often, also, at work to produce a still greater blood pressure on the vessels of the kidney, namely, the hypertrophied condition of the heart, and the dry and unperspiring state of the skin.

Strong action of the heart without evidence of valvular disease, is one of the commonest features of advanced renal disease, and was well known to Dr. Bright.[a] Whether, as that great physician puts the inquiry, the cause of the hypertrophy and strong action, be the altered quality of the blood irritating the left ventricle directly to increased action; or whether it be consequent upon the minute and capillary circulation, so as to render greater action necessary to force the blood through the distant sub-divisions of the arterial tree, as Johnson believes is the cause, I shall not delay to inquire, nor pretend to decide. The fact suffices, and, as Dr. Parkes remarks,[b] on the warrant of the observations of Ludwig,

[a] Guy's Hospital Reports, Vol. i.
[b] *Op. cit.* p. 99.

Goll, Dornbluth, and Heynsius, that other things being equal a strongly acting heart will furnish urine more abundant in water, and richer in solids, than a feeble heart, I can conceive, that although but a small portion of the kidneys is left, and that portion chiefly the medullary, yet, most probably, this strong action of the heart will tend, in addition to other forces, to the production of polyuria in cirrhosis.

In the amyloid kidney, polyuria is more easily accounted for, for besides the compensatory work, which we believe (for reasons stated above) the medullary portion of the kidney takes on it, it is extremely probable that through the Malpighian or other capillaries, a transudation of albuminous fluid takes place similar to the profuse watery diarrhea we occasionally observe in the more advanced stages of the disease.

On a Case of Malignant Disease of the Thorax and Stomach. By Thomas Wrigley Grimshaw, M.D. (Dub.); Fellow of the King and Queen's College of Physicians; Physician to, and Lecturer on Materia Medica in, Dr. Steevens' Hospital; Physician to Cork-street Fever Hospital.

The chief interest of the following case arises from the difficulties presented in determining the true nature of the affection under which the patient laboured during life.

The patient was a well-known medical practitioner of the southern suburbs of this city, aged fifty-one years. His previous medical history presented nothing particular, excepting an attack of fever during the great famine fever. He occasionally suffered from severe bilious attacks, and latterly from bronchitis. About two years ago he received a slight knock on the shin; this was followed by sloughing, and left an unhealthy ulcer, which for a long time remained unhealed. For this affection he was treated by the late Mr. Maurice Collis. The indolence and almost malignancy of this sore pointed to an impaired constitution. I may say his health decidedly failed from this time; he began to suffer from a constant cough, aggravated by attacks of bronchitis, and considerable irritability of the stomach. He had a great objection to have his chest examined, and indeed it was not examined until the disease was far advanced. By the advice of his professional friends he tried quiet and change of air—went to the County Wicklow, North Wales, and several other localities. These changes produced but temporary relief, and towards the end of last summer, after returning from a visit to North Wales, he became decidedly worse, got a severe attack of bronchitis, accompanied by great irritability of stomach. Up to this time (the end of August, 1869) he had been treating himself, sometimes taking the suggestions of his professional acquaintances. At this date he sent for

an eminent physician in this city, who pronounced his case to be probably tubercle of the left lung, softened at the apex, and aggravated by an attack of bronchitis. On the 2nd of September I was called to see him, and found the following symptoms :—Considerable prostration ; pulse 100, moderately strong; cough ; occasionally febrile attacks; great sweating at night; intense irritability of stomach, everything being vomited ; the slightest attempt to cough produced vomiting; considerable pain in the back between the shoulders, but more especially towards the left side of the spine, and some pain along the margin of the ribs on the right side; bowels irregular, but at this date rather confined, and discharges of a clay colour; generally lies on the left side. A physical examination of the chest showed on the left side complete dulness from apex to base, increased vocal fremitus, respiration inaudible except a very slight vesicular sound in front, below the clavicle. In right lung the respiration was puerile, mixed with some bronchitic sounds, and prolonged expiratory murmur, preternaturally clear on percussion. The heart sounds were healthy, but weak. (I should here mention that the patient was an inveterate smoker.) Nothing abnormal could be felt in the abdomen, excepting that the edge of the liver was a little below the margin of the ribs. This condition, had, however, been noticed during his attack of fever twenty years before, and had been present ever since.

The diagnosis I arrived at, after due consideration, was cirrhosis of the left lung.

As to treatment, attention was at first directed to the irritable stomach. A small dose of blue pill and a prussic acid mixture were administered, lime water and milk were ordered as a drink, and a small blister dressed with morphia was applied to the epigastrium. Considerable improvement followed this treatment.

On September the 6th I met an eminent physician in consultation, who found pretty much the same class of symptoms I have just detailed, but less intense than on the 2nd of the month.

After examining the patient, this gentleman pronounced the case to be one "of a very grave nature, and that the patient would die of it," but declined to give a more detailed opinion until he had seen or heard more of the case. From time to time I reported the progress of the case, and after about a month had elapsed this physician stated that he considered the case to be one of cancer of the lung and stomach, and that the patient would *die hard.* The treatment I have just detailed was pursued, with but slight alteration, for about a fortnight, at the end of which time the patient was so much improved that he was able to get up, sit in an adjoining room, and employ himself for several hours a day calculating statistical tables for the Government, but still any exertion produced great weakness. He still complained of pain in his back, which was slightly relieved by counter-irritation. This improvement

continued for some little time, and he got so well that he walked round his garden, drove out, and transacted some business matters in town. During the second week in October he got hoarse, and for one day nearly lost his voice. A laryngoscopic examination showed some thickening and redness of the mucous membrane of the larynx. This hoarseness gradually went off, and was followed by pain in swallowing, described by the patient as very low down, and amounting to but a slight soreness. This was in the last week of October. From this time all improvement ceased; from spending several hours a day up, he was gradually out of bed for shorter and shorter periods until the beginning of November, from which time he might be said to have been completely confined to bed. His great complaint was now intense pain in the back and in the left hypochondrium, frequent attacks of vomiting, difficulty of swallowing any but the softest solids. The patient now subsisted entirely on egg flip, milk with lime water, and beef-tea.

On the 23rd of this month I met in consultation another eminent physician, who, having examined the patient, pronounced him to be labouring under aneurism of the aorta pressing on the left bronchus and œsophagus, announcing his belief that the patient would die suddenly, and recommending that any important business or family matters should be arranged as quickly as possible. The patient gradually got worse. He became more emaciated and weaker, his difficulty of swallowing increased, the paroxysms of pain became more frequent and intense, retching was constant, but nothing was vomited but a little mucous, tinged in a few instances with blood. No sedatives had *much* influence; he seldom obtained sleep for more than an hour at a time, and then only by large doses of sedatives, the most useful of these being opium; subcutaneous injections of morphia up to a grain and a half nearly failed to give relief. The greatest relief was obtained by the almost constant inhalation of chloroform, and chloroform applied externally, with or without morphia, gave some relief; this state of things continued gradually getting worse, until the 26th of December, when he became delirious, apparently suffering much pain. He died the following day. I must mention that for the last six weeks of his life the patient could not lie on the left side, owing to the intense dragging pain he felt when in that position. He described the pain as if his liver were being dragged from the right towards the left side.

With consent and wish of the patient's friends, a *post mortem* examination was made 60 hours after death. The body was greatly emaciated, there being scarcely a trace of fat. On opening the chest, the left lung was apparently absent. Some fluid was found in the left pleural cavity, and a tumour somewhat larger than a shut fist was found occupying the position of the root of the left lung; the outer and anterior part of this tumour was composed of carnified lung, the posterior and internal part,

which was adherent to the spine, filled up the arch of the aorta, and passed insensibly into the carnified lung. It consisted of a white brain-like mass, the consistence being uniform and somewhat harder than brain. This mass enclosed and completely obliterated the left bronchus. The right lung was somewhat emphysematous; the heart was of natural size, paler and softer than natural; the aorta perfectly healthy, and its calibre not larger than natural.

On removing the heart and lungs several small masses of encephaloid matter similar to that involving the root of the lung was found lying along the œsophagus. The lower end of the œsophagus was found to be involved in a mass of this cerebriform substance, the mass extending down behind, and involving the superior and posterior part of the stomach. On opening the stomach it was found to contain some coffee-ground matter, but no ulcer could be seen. The little finger could scarcely be passed into the œsophagus. The disease involved a few mesenteric glands, and a small portion of the large end of the pancreas. The liver was somewhat larger, and its anterior edge harder than natural, but no deposit similar to that found in the chest and stomach was discernible in this organ.

Microscopical examination demonstrated the tumours to consist of a fine granular matter; no cells could be discovered in any, even the hardest parts. My friend Dr. Robert M'Donnell also examined this substance, and agrees with me in considering it to be cerebriform cancer.

As I stated at the commencement of these remarks, the chief interest of the case lies in the difficulty of diagnosis, and it is not a little humiliating to find such a difference of opinion concerning a case where *post mortem* examination showed such well marked and extensive disease. There were four well marked opinions concerning the disease under which this patient laboured.

1st. Tubercle of the lung.—This opinion was held by an eminent physician of this city, and by two other professional men who have had large experience, and whose opinions are respected.

2nd. Cirrhosis of the lung.—This was my own opinion, and in it I had the concurrence of an able and acute medical officer of her Majesty's navy, who had the same opportunities of observation as I had.

3rd. Cancer of the lung.—Pronounced by an eminent physician and distinguished member of this Society.

4th. Aneurism of the aorta.—By another eminent and distinguished member of this association.

Several questions at once occur to us, on learning the details of such a case. How could such a variety of opinions prevail? and how can this case lead to more certain diagnosis in future and similar instances? The question of tubercle I set aside on the grounds of no family history, too little respiration (in fact it was almost, if not altogether, absent in

the affected side), no phthisical expectoration of any kind. I believe the opinion was formed on the grounds of something very closely allied to, if not identical with, hectic fever, general debility, some doubtful crepitation under left clavicle, dyspepsia, retching, and irregular bowels. I believe there were fair *rational* grounds for this opinion.

The opinion of cirrhosis of the lung was formed upon the extensive absence of respiration from the left side; absence of any of the expectorations indicative of phthisis, with many of the rational signs of that disease present.

The diagnosis of cancer was grounded more, I believe, upon the method of exclusion than upon the character of the physical signs, and by the general look of the patient (which, I must confess, did not seem to me sufficiently malignant to attract my notice until it was recognized and remarked upon by my more experienced senior); by the intense paroxysms of pain, and soreness of swallowing. It was this soreness afterwards becoming an obstinate and painful obstruction, that ultimately led me to agree that the disease was of a malignant nature, and had involved other organs than the lung.

The opinion, aneurism of the aorta, was chiefly founded upon the detection of two centres of pulsation in the chest, together with a doubtful aortic murmur. My reason for not concurring in this opinion was, that I believed an aneurism of the aorta of sufficient magnitude to completely close the left bronchus, and produce such obstruction to swallowing, would produce some, or probably a considerable number, of those head, neck, and such symptoms with which we are familiar in cases of thoracic aneurism. The *post mortem* examination, however, showed what fair grounds there were for this belief in thoracic aneurism. The tumour involving the root of the left lung almost closed up the arch of the aorta, and encircled to some extent its descending portion, gluing together all the adjacent parts, thus forming a mass which must necessarily be moved by each pulsation of the aorta, and perhaps causing sufficient pressure upon the great vessels to produce a murmur. I did detect this pulsation, but never the murmur; the pulsation often seemed insignificant, and, taken with other symptoms, insufficient to denote aneurism.

In a case of this sort it is impossible to fix, with any degree of accuracy, the first locality attacked by the disease. In this case I believe, however, it commenced in the glands near the root of the lung, and must therefore be considered to be more of the nature of mediastinal than pulmonary tumours, and we know the difficulties attending the diagnosis of mediastinal tumours.

I am afraid the question of how to diagnose a similar case cannot be definitely assumed; nevertheless, I think this case may be *suggestive* as to the determining the diagnosis of obscure diseases within the thorax, and it is with this hope that I have laid it before the Society.

On General Dermatitis, or Pityriasis Rubra. By J. Hawtrey Benson, M.B., L.K.Q.C.P.I.; and Walter G. Smith, M.B., F.K.Q.C.P.I.

The following is a description of a case of an extremely rare form of skin disease. It is an example of general superficial inflammation of the skin, or, according to Hebra's nomenclature, *Pityriasis Rubra.* It is the same which Erasmus Wilson, in his last edition, terms *Pityriasis Rubra Foliacea.*

As a proof of its rarity, we may mention that up to the time the second volume of the English translation of Hebra's work on *Diseases of the Skin* was published, namely, in 1868, that great dermatologist had seen but *three cases.* When Erasmus Wilson published his work in 1867, he had met with but *two cases.* Since that time, however, Mr. Wilson has published a third case of the disease in the *Medical Times and Gazette* for January 29th of this year, and there he calls it *Dermatitis Exfoliativa.* In *Guy's Hospital Reports* for the year 1861, Dr. Wilks published a case which occurred in the practice of Dr. Owen Rees, under the name of "General Inflammation of the Skin, or Dermatitis," which appears to correspond with the *Pityriasis Rubra* of Hebra. Devergie, too, has met the disease. But, with these four exceptions, we are not aware of any other author having described it. The first writer who mentioned the name of *Pityriasis Rubra* was Bateman, in his continuation of Willan's work, *On Cutaneous Diseases*, but he gave no definition of it applicable to all cases of this affection, and which would exclude other complaints resembling it. In fact, Hebra concludes that Bateman never saw the disease in its general form; and that, in like manner, a number of others who have since written on the subject of skin diseases, including Biett, Green, Cazenave, Schedel, Chausit, Gibert, Frank, Alibert, Fuchs, Duchesne-Duparc, and Rayer, had never seen the disease in the *general* form which he himself describes, and of which we are about to give the notes of a case.

T. F., aged forty, a labourer, employed in chemical works, applied for relief at the Adelaide Hospital Dispensary, on October 4th, 1869. Six weeks previously, without any premonitory symptoms, he noticed an eruption of "little red spots" on the trunk and abdomen, which spread rapidly over the whole body. The spots soon became confluent, producing a continuous red surface. This condition was followed, in a few days, by general exfoliation of the epidermis in *large* whitish flakes, which averaged from about the size of a threepenny piece to that of a shilling. The entire surface of the body, from the head to the feet, was of a vivid deep-red colour, which partially disappeared on pressure, leaving a yellowish impression.

On this background the epidermic scales were seen disposed parallel to the lines of flexion. They stood out large, lustrous, very thin—like silver paper—exceedingly abundant, and were easily rubbed off. One edge,

generally the inferior, was free, and was raised a line or two above the level of the skin underneath, while at the opposite border the scale was continuous, with the deep red cuticle as yet undetached. He had a sensation as if the skin was dry and parched, and he felt unwilling to bend his joints lest the skin should "crack," as he expressed it. The thick cuticle of the palms of the hands freely peeled off. Of the true skin there was but little infiltration; no considerable exudation into it, nor on its surface. There was no itching except at night (and even then it was inconsiderable), nor any other unpleasant sensation, except some numbness at the lower part of the abdomen. The conjunctivæ were slightly injected. He never suffered from any form of venereal disease, and was always a healthy man till this eruption appeared. Since that time he had slightly emaciated, and had become so weak that he was confined for the most part to bed. He constantly shivered, and was very susceptible of cold. The pulse was over 100, and compressible; and he complained of nocturnal palpitations. His appetite was not affected. He was ordered a grain of quinine four times a day.

On the 7th he was no better, and as he was not admissible into the Adelaide Hospital, Dr. Smith placed him under the care of Dr. Hawtrey Benson in the City of Dublin Hospital. After a few days rest in bed, the pulse had fallen to 70, and the temperature in the axilla varied from day to day between 99·2 and 99·6. The tongue was abnormally red, especially at the margin and tip. It was partly covered with a thin white fur, which was deficient in irregular patches, disclosing the bright red surface of the tongue underneath. His bowels were regular. The urine was slightly acid, rather scanty and high coloured, and contained an abundance of phosphates, but no albumen nor sugar. But for the redness of the tongue and slight injection of the conjunctivæ, the mucous tracts seemed unaffected by the disease.

The epidermic scales from the body were shed in very great quantities, and reproduced with a rapidity quite remarkable. Every afternoon most of the loose scales were rubbed off in the bath which he took daily, and yet by next morning the scales lying in his night-shirt and on his under-sheet would, at least, fill a four or six ounce measure glass. The nails of both fingers and toes were unaffected throughout. No rhagades appeared in any part. In about a week after his admission he suffered from two superficial abscesses in each axilla, from which he soon recovered.

He was ordered warm, alkaline baths daily, and the application of emollient ointments. These latter were most grateful to him, and he continued the use of the baths and ointments from the commencement to the termination of the treatment. Of internal remedies we at first employed consecutively quinine, iron, the mineral acids, strychnia, and belladonna, but without in the least modifying the disease. On th

contrary, the debility steadily progressed. Disappointed at finding no improvement after about a fortnight's treatment, the patient left the house upon some trivial excuse. However, after a week's enjoyment of home society with starvation and filth, he presented himself again at hospital, considerably worse than when he left it, and was readmitted. While he was out the thick cuticle of the soles of the feet desquamated in large sandal-like flakes.

When he returned we put him immediately upon arsenic, using alternately the liquor arsenicalis (Fowler's) and the liquor arsenici hydrochloricus (de Valangin's).

In a few days afterwards the scales became less numerous and smaller. Next, the skin underneath gradually lost its intense redness; his general health improved, and in less than a month from his readmission, that is, in about seven weeks from the commencement of treatment, or thirteen from the first appearance of the disease, almost every trace of desquamation had disappeared, except about the head; and the skin had almost quite regained its natural colour and pliancy. Scattered over the trunk were left a few brownish-red patches of pigment not altered by pressure, irregularly oval and about the size of a sixpenny piece. These patches were longer surmounted by scales than the neighbouring skin.

The first thing that strikes us in this case is the very slight constitutional disturbance attending such very considerable lesion of the *entire* cutaneous surface. The patient ate and slept well, and was free from pain or unpleasant sensation, with the exception of slight pruritus at night. In fact, the only considerable constitutional symptom was debility, joined with slight increase of temperature, and slight acceleration of pulse.

As to diagnosis, both Hebra and Wilson enter at some length into the question of the differential diagnosis between this disease and *eczema*; but the boundary-line seems pretty well defined. The absence of moist exudation, and of distressing pruritus; the characters of the epidermic lamellæ—large, thin, profuse, and exhibiting no trace of corpuscular matter—as well as the universality of the disease over the whole body, no external part escaping, are sufficient to exclude *eczema* in any of its stages. Devergie, when speaking of *pityriasis rubra*, lays it down that no other disease is ever universal over the whole cutaneous surface, except *acute lepra*.

As a full consideration of the points of differential diagnosis between *pityriasis rubra* and other diseases likely to be confounded with it, would be altogether too tedious, we will merely quote a few lines from Hebra bearing on the question. "As appears from its name," that author says, "the affection which is termed *pityriasis rubra* consists in a reddening of the skin, which is also covered with scales. But, as is well known, these characters are likewise found in other dermatoses, such as *psoriasis*, *lichen*,

eczema, and *lupus erythematosus;* and therefore we have to give some additional symptoms belonging only to the disease in question, and absent in those which thus resemble it. In doing this, however, we must rely not so much on positive as on negative characters. In the above-named affections, besides the redness and the scales, there are always other symptoms. Either there is infiltration of the cutis, with or without the presence of fissures; or there is oozing of a fluid secretion; or there is severe itching, which leads inevitably to scratching, and consequently to the existence of excoriation; or, lastly, the disease occupies some particular region, or presents certain peculiarities in its course. But in the *pityriasis rubra* all these are absent. In it there is nothing more than an intense redness diffused over a large part of the skin, or even universal, disappearing beneath the pressure of the finger (when it gives place to a yellowish colouration), and accompanied by the presence of fine white loosely-adherent scales, which result from the constant shedding of the most superficial layer of the cuticle."

According to Hebra's experience cases of this disease are very hopeless. Each of his three cases ended fatally, after years of duration. The disease was apparently uninfluenced in the least by any internal remedies. Arsenic, tincture of cantharides, sulphur, antimony, decoctum lignorum, sarsaparilla, iodine, and mercury, all failed alike to produce any change whatsoever in the characters of the disease. However, by the repeated employment of tepid baths, and by the application of oils and emollient ointments, the disease "has, to a certain extent, been modified" under his observation. But in these countries the disease does not seem so hopeless. One of Wilson's three cases ended in recovery; another ended fatally through the effects of intercurrent bronchitis; and the third was still labouring under the disease at the time he published the account of it in the *Medical Times and Gazette*. Dr. Owen Rees' case ended favourably. Our case also recovered completely, and we are inclined to give a very large share of the credit, if not the entire, to the use of arsenic.

Report of Cases of Scarlatina followed by unusual Sequels. By S. M. MacSwiney, M.D., Physician to Jervis-street Hospital.

Case I.—On 11th December, 1869, I was asked to attend Monsieur Ade V——, a Belgian young gentleman, aged eighteen years, for an attack of fever. At my first visit I learned that he had been complaining of illness for the previous twenty-four hours; and that he had had headache, sick stomach, and shiverings. I found him in the following condition:—His pulse was 120 in a minute; his tongue was densely coated white; he had thirst, and was restless and indisposed for sleep. He complained of sore throat, and upon inspection, I found the tonsils much swollen,

and their surface, as well as that of the entire pharynx and mouth, was of a deep scarlet colour. There were no ulcerations in any part of the mucous membrane. Upon examining the skin of the body, I ascertained the existence of several large sized patches of the characteristic scarlet eruption over different parts of the trunk. It was punctiform, bright red in colour, diffused. The separate patches coalesced next day, and covered the entire trunk of the body. The diagnosis was easy enough; he had a mild attack of scarlatina, and went through it without any unusual symptom until the tenth day. His pulse had fallen to 80; his tongue had quite cleaned; the eruption had entirely disappeared; the urine was clear, and non-albuminous—in a word, he seemed to be quite convalescent. At this time, however, namely, the tenth day after the appearance of the eruption, his stomach became sickish; his pulse suddenly rose to 120 again; he spent a sleepless night, and was thirsty. The urine he passed was dark, almost black indeed, in colour; and submitted to tests, it was found not to contain blood; his conjunctivæ were tinged with yellow, and there was an icteric hue over the skin of the body generally. On the next day he had well-marked jaundice. This showed all the usual symptoms of an ordinary attack of that morbid state, and lasted for about eight days, after which time all the symptoms subsided; he made a rapid and good recovery, and has remained in excellent health up to the present time.

Case II.—On 1st January of this present year, I was asked to see, in consultation, two young ladies, sisters, at Booterstown. I found that they were patients of my friend Dr. R. Griffin, who had been for some days in attendance, and, recognizing the nature and gravity of their state, had now called for professional assistance. I was thus associated in attendance with him, and continued to visit the invalids for one month, when I left them convalescent. *History.*—Dr. Griffin informed me that the younger sister, A——, had been attacked with shivering, intense headache, and fever, five days before my first visit. When he saw her two days after the commencement of her illness, he was struck with her extremely weak state. His attention having been directed to her throat, he found it greatly swollen and inflamed; her tongue was also much swollen and coated; ulcerations prevailed extensively in the mouth and throat; there was, at the same time, a good deal of general fever. Suspecting at once the nature of the case, Dr. Griffin made a most particular search for the scarlatinal rash, but failed to observe a trace of redness; and was assured, moreover, that there had not been any eruption on any part of the body previous to his visit. On the occasion of my first visit, the patient A—— presented all the appearance of the most alarming depression; her face had a pale, wasted, and pinched look; her pulse was very rapid; could scarcely be counted indeed, and was

extremely feeble; she spoke with great difficulty, in consequence of the inflamed state of her tongue, mouth, and pharynx; her voice was indistinct and feeble; she could not open her mouth more than half an inch wide; the skin of the neck was dusky-looking, and it and the underlying tissues were greatly swollen. Inspecting the mouth as well as I could, I made out that the tongue, hard and soft palate, and the cheeks internally, were dry, glazed, and covered with a tough dark-brown crust. The surface of the tonsils and adjacent parts was in a greatly disorganized, ulcerated, yellowish-grey sloughy state. There was *sordes* on the teeth and lips; and she was so weak, that the assuming of the semi-erect position for the purpose of being examined, very nearly caused syncope. Her thirst was extreme. The history of the case, the symptoms which had ushered in the sickness, and the nature and appearance of the lesion of the throat, enabled me to fully confirm Dr. Griffin's diagnosis of "scarlatina maligna sine eruptione." Notwithstanding that the length of time which had elapsed since the invasion of the disease was sufficient to admit of the disappearance of any cutaneous eruption, if such had ever been present, I still thought it my duty to personally investigate this point; I therefore repeated Dr. Griffin's search for a rash, and with a like negative result. I was also, as he had been, informed by the nurse, that she had previously looked all over the body, and that there had not been an eruption at any period of her illness. With the utmost difficulty the patient struggled through this severe attack, being, indeed, at times, in an almost hopeless condition, until the twelfth day of her illness, when being generally better than she had, as yet, previously been, two noteworthy observations were made, viz., (*a*) the skin at the root of the nails of both hands began to crack and fall off; and (*b*) severe pains, which we looked upon as being rheumatic in character, were felt by the patient in her shoulder, and other joints. The process of desquamation, which we have seen, was thus commenced at the ends of the fingers, went on increasing until it involved the epidermis of the entire body, large flakes coming off in some parts. The rheumatic pains yielded to treatment; but on the fifteenth day of her sickness, the patient complained of pain in the groin and left leg, with inability to move the limb.

Upon examination of this, we ascertained the following to be the state of affairs. The thigh and leg were semiflexed and somewhat swelled. The skin over these parts was white, bloodless, and shining. There was pain and thickening in the course of the femoral and crural vessels. Pressure on the back of the calf of the leg, in the centre, could not be borne, whilst pressure in the course of the femoral vein was most unwillingly permitted to be made. In each instance, the knotted, cord-like feel of the thickened vessel could be easily made out. In fact, the vein, and its branches where they could be felt, were completely closed by coagulum or adherent fibrine. There was, as I have said, pain complained of; and

this was, distinctly, of two different kinds, viz. (1), a dull pain, which was constantly present, and (2), a violent pain, which was intermittent, and caused the patient to scream out whenever it recurred. The affection progressed in severity day by day, the vein of the right leg also soon became engaged in the same way; the swelling was now enormous, there was great œdema, and the pain aggravated by the slightest movement of the limbs, was, even during periods of perfect rest, intense; large superficial veins, full of dark blood, made their appearance on the thighs, abdomen, and pelvis, and carried on the collateral venous circulation. Her shoulders and hips suffered from the decubitus which was constantly on her back; she became haggard; sordes continued to form upon her lips and teeth; she had the "facies hippocratica;" her pulse was scarcely perceptible; and we greatly feared she would never survive through this almost hopeless state. However, just as matters appeared to be at the worst, she began to mend, and ultimately, on the 32nd day of her illness, was convalescent.

Case III.—S., sister to the patient whose case has just been related, was attacked two days later than A., with headache, thirst, fever, and sore-throat. She had no "rash" on her body, a fact which was ascertained with certainty. She had always been a weak, delicate girl, and from the outset of her illness, she was greatly prostrated. Her pulse was quick and extremely weak; she could not sleep, and was unable from total disrelish of food, as well as from the soreness of her mouth and throat, to take much nourishing drinks. She swallowed, indeed, little else save iced water, barely coloured with claret, and small quantities of milk. Her tongue was dry, brown, and rough in the centre; white and moist at the edges. She slowly progressed, in an unsatisfactory way through her illness, when, on the eighth day symptoms of *typhoid fever* presented themselves. She had tympanitis; she complained of much pain in the ileocecal region (which was tumid from air), and when pressure was made in that situation, gurgling was distinctly elicited. Her bowels were loose, being opened three or four times in the twenty-four hours. There were some eight or ten rose coloured spots scattered over the surface of the chest and abdomen. Her weakness and prostration were extreme. At no time were there any symptoms of disturbance of the intelligence. The typhoid fever now became the prominent disease to be attended to; she passed through it moderately well, the chief danger being from weakness arising from her inability to take any sufficient amount of suitable nourishment. It was very interesting to watch, as we did, the desquamation of the epidermis at the ends of the fingers, where the matrix of the nails is inserted, due to the scarlatina, going on at the same time that the patient was passing through the stages of a somewhat mild attack of undoubted typhoid fever. Ultimately, desquamation

having become general over the body, she recovered strength, and became entirely convalescent. I will now sum up, in a few words, the facts which I deem of interest in these cases. I think the rarity of the *sequels* to scarlatina met with by me in the three patients, whose histories I have just recorded, will be conceded :—

1st.—In case No. 1, jaundice supervened. This affection does not often, undoubtedly, occur as a sequel to the disease, and when it does occur must be due to one of three causes, viz. :—(*a*) Congestion of the liver. (*b*) Catarrh of the bile ducts. (*c*) Blood poisoning. Dr. George Harley (*Pathology and Treatment of Jaundice*, p. 93), relates a case of scarlatina in which jaundice occurred. Dr. Graves, in his *Clinical Lectures*, Vol. i., p. 453, mentions two cases of scarlatina in which jaundice was a complication. Murchison (*Lectures on Diseases of the Liver*, p. 388), observed this sequel of scarlatina in five instances. Three out of these five cases had a fatal termination. Other observers, also, have met with the affection in this class of cases, but it is, confessedly, an unfrequent occurrence.

2nd.—In case 2, phlebitis of a very severe nature occurred. I have not met with any record of this serious complication of scarlet fever having been met with before. Sir William Jenner, indeed (Gulstonian Lectures, 1853), pointed out the occurrence of a species of pyemia found to follow scarlet fever and other acute affections. And indeed we had felt, in the course of this disease, that it was quite possible, and even not unlikely that pyemia might supervene. But I have no where heard or read of *phlebitis* after scarlatina.

3rd.—I make the same remark respecting case three where scarlatina was followed by typhoid fever. This complication does not appear to have been observed previously. The nearest approach to a recognition of the relation of these two diseases to each other, as met with by me in this instance, is made by the late Mr. Hillier, who, at page 308 of his book on *Diseases of Children*, says "scarlet fever in the course of typhoid fever has been noticed in a number of cases." But here the scarlet fever is the complication ; whilst in my case it was the primary disease.

4th.—Finally, the two cases last detailed may be fairly set down as instances of scarlatina without the scarlet rash. Sir W. Jenner, indeed, doubts if this state is ever really met with, and suggests that in the supposed instances of it which have been recorded, the rash was slight and evanescent, and had thus escaped observation. But in reply to this it is to be remembered that many experienced physicians have testified to the correctness of the statement of its occasional occurrence. I will name but one, the illustrious *Trousseau*, who teaches that cases of scarlatina without rash are often met with.

It was, to our minds, quite satisfactorily made out that there had not, at any time during the sickness, been an eruption in the case of the two

sisters, in both of whom, however, extensive desquamation followed in due course. And Dr. Graves, who maintains the reality of scarlatina without an eruption, gives cases which conclusively establish the fact of its sometimes occurring. It was on account of the several points thus briefly enumerated that I thought it would be interesting to the members of the Medical Society to hear the detail of these cases.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

THIRTY-FIRST ANNUAL SESSION.

Dr. Ringland, President.

On the Muscular Forces employed in Parturition, their Amount and Mode of Application. By the Rev. Samuel Haughton, M.D., Dubl., D.C.L. Oxon.; Fellow of Trinity College, Dublin.

In the first stage of natural labour, the involuntary muscles of the uterus contract upon the fluid contents of this organ, and possess sufficient force to dilate the mouth of the womb, and generally to rupture the membranes. I shall endeavour to show that the uterine muscles are sufficient, and not much more than sufficient, to complete the first stage of labour, and that they do not possess an amount of force adequate to rupture, in any case, the uterine wall itself. In the second stage of labour, the irritation of the fetal head upon the wall of the vagina, provokes the reflex action of the voluntary abdominal muscles, which aid powerfully the uterine muscles to complete the second stage, by expelling the fetus. The amount of available additional force given out by the abdominal muscles admits of calculation, and will be found much greater than the force produced by the involuntary contractions of the womb itself.

The mechanical problem to be solved for both cases is one of much interest and some difficulty, as it is the celebrated problem of the equilibrium of a flexible membrane subjected to the action of given forces. It has been solved by Lagrange (*Mecanique Analytique*, p. 147). In the most general case of the problem, the following beautiful theorem can be demonstrated. Let T denote the tensile strain acting in the tangential plane of the membrane, applied to rupture a band of the membrane one inch broad; let P denote the pressure resulting from all the forces in action, perpendicular to the surface of the membrane, and acting on a surface of one square inch; and let ρ_1 and ρ_2 denote the two radii of principal curvature of the membrane, at the point considered. Then we have the following equation—

$$P = T \times \left(\frac{1}{\rho_1} + \frac{1}{\rho_2} \right) \qquad (1)$$

If the surface, or a portion of it, become spherical, the two principal curvatures become equal, and the equation becomes—

$$P = \frac{2T}{\rho} \qquad (2)$$

In the case of the uterus and its membranes, the force P arises from hydrostatical pressure only, and is therefore easily measured, and the supposition of spherical curvature is approximately admissible.

In the first stage of labour, the contraction of the muscular walls of the uterus compresses its liquid contents, and the membranes are pushed through the mouth of the womb, bulging out into the segment of a sphere, and are in most cases ruptured by the pressure of the liquor amnii contained in them.

It is evident from the form of the gravid uterus that its curvature is greatest near its mouth, and equation (2) shows that for a given hydrostatical pressure, the tensile strain is proportional to the radius of curvature; hence this strain will be greatest at the fundus of the uterus. If we assume the shape of the uterus to be that of a prolate ellipsoid, whose longer diameter is 12 inches, and shorter diameter 8 inches, its mean curvature will be that of a sphere whose diameter is 9·158 inches.

The volume of the gravid uterus is found from the expression—

$$\text{Volume} = \frac{4}{3}\pi ab^2; \qquad (3)$$

in which a and b are the semiaxes, and π is the ratio of the circumference of a circle to its diameter; substituting for a and b their numerical values, we find the contents of the uterus to be 402·13 cubic inches.

The surface of the gravid uterus may be found from the equation—

$$\text{Surface} = \frac{2\pi ab}{e}\left(\text{Sin}^{-1}e + e\sqrt{1 - e^2}\right); \qquad (4)$$

in which e is the eccentricity of the generating ellipse. If the numerical values be substituted in this expression, it will be found that the surface of the uterus is 270·66 square inches.[a]

Some highly interesting conclusions may be drawn from the preceding calculations, combined with the weight of the total muscular tissue of the uterus. Heschl estimates the weight of the uterine muscles at from 1 lb. to 1·5 lb. Montgomery found the muscles of the gravid uterus to weigh 1·5 lbs.; and Levret estimates them at 51 cubic inches, which, with a

[a] Levret estimates the contents of the gravid uterus at 407 cub. in., and its surface at 339 sq. inches. Poppel estimates the contents at 300 cub. in., and the surface at 210 sq. inches.

specific gravity of 1·052, I find to be equivalent to 1·93 lb. Taking the mean of these estimates, we have—

Weight of Muscular Fibres of Gravid Womb.	
Heschl,	1·25 lb.
Montgomery,	1·50 „
Levret,	1·93 „
Mean,	**1·56** lb.

If we now suppose this quantity of muscle to be spread over the entire surface of the uterus, we find—

$$\left.\begin{array}{l}\text{Mean thickness of mus-}\\ \text{cular wall of uterus,}\end{array}\right\} = \frac{1{\cdot}56 \times 7000 \times 1000}{252{\cdot}5 \times 270{\cdot}66 \times 1052} = 0{\cdot}1519 \text{ inch.}$$

If we suppose a ribbon, one inch in width, to be formed from the wall of the uterus, its thickness will be 0·1519 inch; and as each square inch of cross section of muscular fibre is capable of lifting 102·55 lbs., we find for the greatest tensile force produced by the contraction of the uterine muscles.

$$\left.\begin{array}{l}\text{Tensile strain of uterine}\\ \text{wall per inch}\end{array}\right\} 102{\cdot}55 \times 0{\cdot}1519 = 15{\cdot}577 \text{ lbs.}$$

Substituting this value of T, in the equation (2)—

$$P = \frac{2T}{\rho}$$

and for ρ, its mean value 9·158 in., we obtain the maximum hydrostatical pressure inside the gravid uterus producible by the contraction of its muscular fibres.

$$\left.\begin{array}{l}\text{Maximum hydrostatical pressure}\\ \text{produced by uterine contraction}\end{array}\right\} = \frac{2 \times 15{\cdot}577}{9{\cdot}158} = \mathbf{3{\cdot}402} \text{ lbs.}$$

One hundred experiments were made by Duncan and Tait, upon the hydrostatical pressure necessary to rupture the membranes which contain the liquor amnii, which are recorded in Dr. Duncan's book[a] (p. 306–311.) The greatest pressure observed was 3·10 lbs., and the least was 0·26 lb.; and I find that the mean rupturing pressure of all their experiments was **1·2048** lbs.

Combining this experimental result with the calculation already given, of the amount of pressure producible by the muscular tissue of the womb, we may conclude that the uterine muscles are capable of rupturing the membranes in every case, and possess, in general, nearly three times the amount of force requisite for this purpose.

The weight of muscle in the virgin uterus is about one-and-a-half ounces, and this muscle is increased to one-and-a-half pounds during pregnancy; that is, it is increased about sixteenfold. If ever there was a muscular system produced to effect a specific object, the uterine muscle

[a] Researches in Obstetrics. Edinburgh, 1868.

may be regarded as such; and if the principle of least action in nature be valid, we should expect such a muscle to be developed only to the extent required, and not beyond it. Considered from this point of view, the preceding calculation must be regarded as interesting, for it shows that the uterine muscle possesses a force of 3·4 lbs., intended to overcome a maximum resistance of 3·1 lbs. It would be a waste of power to endow the uterus with more force than I have shown it to possess, for it is not necessary that the uterus should complete the second stage of labour, as the abdominal muscles are available for this purpose; so that by using them, and not giving the uterus more force than is absolutely necessary for the first stage of labour, an admirable economy of muscular power is effected.

Viewed in connexion with the doctrine of final causes, the uterine muscle presents a subject well worthy of contemplation. It is plainly necessary that the first stage in the expulsion of the fetus should not be entrusted to a voluntary muscle, and hence an involuntary muscle is gradually provided, which takes the initiative and commences the process of parturition, the completion of which is then accomplished by the aid of voluntary muscles, to the employment of which, at this stage, no moral objection can be raised. It is also necessary (if the Contriver be allwise, or if the principle of least action in nature be true), that the involuntary muscle, so produced, should not possess more or less force than is requisite for its purpose. The uterine muscle does not grow, to meet a growing resistance (as happens frequently in other cases), and its precise degree of strength cannot be produced by a tentative process; for in healthy gestation, the uterine muscle never tries its force against the membranes it is called upon to rupture until the actual period of parturition has arrived. We witness, therefore, the instructive spectacle of a growing force tended to overcome, at a definite future period, a definite resistance, against which it never measures its strength until the actual moment of conflict has arrived.

The mind must be singularly constituted which fails to see in such an arrangement the clear evidence of a Divine Contriver, adapting means to an end foreseen by Himself.

The extreme force of uterine contraction produces a pressure of 3·402 lbs. per square inch, which is equivalent to a pressure of 54·106 lbs. acting upon a circle of 4½ inches in diameter, which is assumed as the average area of the pelvic canal.

The maximum force used to expel the fetus, by both uterine and abdominal muscles combined, is estimated by Joulin,[a] by forceps experiments made on the dead body, at 110·23 lbs., a result which is regarded by Dr. Duncan[b] as too large. Dr. Duncan himself considers 80 lbs. as

[a] Traité Complet des Accouchements, p. 447.

[b] Researches in Obstetrics, p. 323.

the maximum force ever employed in difficult cases. This would correspond with an hydrostatical pressure inside the uterus of 5·03 lbs. per square inch, which is greater than the uterine muscles, unaided, are capable of producing.

Dr. Duncan's experiments on the strength of the uterine wall itself show that an hydrostatical pressure of from 17 to 25 lbs. per square inch is necessary to its rupture; a result which conclusively demonstrates, when taken in conjunction with the preceding calculations, that the uterus can never destroy itself by its own contraction. It would, indeed, be *a priori*, ridiculous to suppose that Nature would produce an involuntary muscle capable of destroying itself, and so causing the death of its owner by its own blind force.

In concluding my remarks on the uterine muscles, I may observe that the arrangement of these muscles on various parts of the organ, affords a confirmation of the accuracy of Lagrange's principle as applied to them. It is well known that these muscles attain their greatest thickness at the fundus of the womb, where they are fully double the thickness of the muscular layer at the neck of the womb. Now, so long as the contents of the womb are fluid, or semi-fluid, a constant pressure is exerted perpendicularly to all portions of its walls, requiring, by Lagrange's theorem, a tensile muscular force varying as the diameter of curvature at each point. From observations on the curvature of the gravid uterus, it may be inferred that the diameters of curvature at the fundus and neck of the womb, are nearly in the proportion of 29 to 17. When the womb is emptied of its contents, and the uniform hydrostatical pressure inside is removed, the uterine walls contract with degrees of force proportional to the diameters of curvature they possessed in their former condition; the fundus contracting with twice the force of the parts near the neck, an action eminently calculated to promote the safe and speedy reduction of the organ to its diminished bulk, without risk of inversion.

It is worth while to consider for a moment, the explanation offered by the Lamarckian theory of natural selection, of the relation between the strength of the uterine muscles and the resistance of the membranes they are required to rupture. According to this theory, an animal that possessed such a proportion as I have shown to exist between the force and resistance to be overcome, must be the descendant from ancestors who attained this proportion accidentally, and transmitted it to their posterity, who thus acquired an advantage over their neighbours, in the competition for life. Thousands and tens of thousands of those who have preceded us, must have perished in the race, from not having had the precise balance between uterine force and resistance, which we happily possess; and we may regard ourselves as the survivors of a contest in which the slain greatly outnumber the victors. According to this school of philosophy, an economy of two per cent. of wax will make a bee; and a

saving of a quarter ounce of uterine tissue may, possibly, constitute the sole difference between a Celtic and a Teutonic woman.

Happy is the man whose mother possessed the hereditary tendency to form no more uterine muscle than was absolutely necessary to push him into the world! Happy is the bee who can form her cell with a saving of two grains of wax in the hundred, for she will distance in the race of life her competitors who have not inherited this precious art!

In the second stage of labour, the voluntary action of the abdominal muscles is called into play, to aid the expulsive efforts of the uterine muscles. I have attempted to calculate the force available from the contraction of these muscles as follows:—

The abdominal muscles are four in number, viz.—*rectus abdominis*, *obliquus externus*, *obliquus internus*, and *transversalis*. The last three muscles form curved sheets, acting upon the corresponding muscles of the opposite side by means of tendinous *aponeuroses* which meet in the linea alba, and form the sheath of the vertical *rectus abdominis* muscle. From the arrangement of all four, it is plain that the tensile force of muscular contraction in the curved wall of the belly from the xiphoid cartilage to the symphysis pubis, is to be measured by the sum of the united forces of all the muscular sheets. If we knew the force of each muscle and the principal curvatures of the belly in the middle line, we could calculate, by Lagrange's theorem, the hydrostatical pressure inside the abdominal cavity, and available to expel feces, urine, or a fetus.

In order to ascertain the force of the muscles, I measured carefully their average thickness in three subjects, of whom one was a young woman who had borne children, and the others were men of ordinary size and appearance. The results obtained were the following:—

Thickness of Abdominal Muscles.

	No. 1—Male	No. 2—Female	No. 3—Male
Rectus abdominis, -	0·275 in.	0·29 in.	0·34 in.
Obliquus externus, -	0·200 „	0·25 „	0·19 „
Obliquus internus, -	0·235 „	0·17 „	0·24 „
Transversalis, - -	0·127 „	0·15 „	0·14 „
Totals, -	0·837 in.	0·86 in.	0·91 in.

The average total thickness of the muscular walls is 0·869 inch, which is nearly identical with the measurement obtained from the female subject. It has been often shown that we must add 50 per cent. to the weights of muscles in the dead subject in order to bring them to the living weights. This correction gives us 1·3035 inches for the mean thickness of the muscles causing tension in the central line of the belly, where the forces of all the muscles come into play together. Multiplying this thickness by 102·55 lbs., or coefficient of muscular contraction, we find

$$T = 1{\cdot}3035 \times 102{\cdot}55 = 133{\cdot}67 \text{ lbs.}$$

This is the tensile strain producible by the contraction of the abdominal muscles along the curved central line of the belly.

It remains now to ascertain the principal curvatures of the abdominal surface, and to use the equation—

$$P = T\left(\frac{1}{\rho_1} + \frac{1}{\rho_2}\right),$$

so as to determine P, the hydrostatical pressure per square inch inside the cavity of the belly, and available, either in whole or in part, for the expulsion of the fetus, during the second stage of labour.

In order to ascertain the curvature of the belly, I made experiments upon three young men, placed lying upon their backs upon the floor, and made them depress and raise the abdominal wall as much as possible. The result was as follows:—Taking a straight line from the upper part of the symphysis pubis to the xiphoid cartilage as the fixed line of comparison, it was found possible to depress the navel one inch below this fixed line, and to raise it two inches above it. When the belly was distended to the utmost, by the action of the abdominal muscles, I measured the longitudinal and transverse curvatures, by measuring the sagittas corresponding to a given length of tangent, with the following result:—

No.	Diameter of Longitudinal Curvature	Diameter of Transverse Curvature
J. G. H. -	22·93 inches.	12·30 inches.
H. O. - -	22·73 „	12·80 „
S. H. - -	22·52 „	12·80 „
Mean, -	**22·727** inches.	**12·633** inches.

The curvature of the distended belly, at the navel, is found to be, from the foregoing measurements—

$$\frac{1}{\rho_1} + \frac{1}{\rho_2} = \frac{1}{11{\cdot}3635} + \frac{1}{6{\cdot}3166} = \frac{1}{4{\cdot}0596}$$

Multiplying this curvature into the tension of the abdominal muscles at the navel, already found, viz., 133·67 lbs. per inch, we obtain finally—

$$P = \frac{133{\cdot}67}{4{\cdot}0596} = 32{\cdot}926 \text{ lbs. per square inch.}$$

This amount of expulsive force per square inch is available, although not usually employed, to assist the uterus in completing the second stage of labour. If we suppose it applied to a surface of a circle $4\frac{1}{2}$ inches in diameter (the usual width of the pelvic canal), we find that it is equivalent to 523·65 lbs. pressure.

Adding together the combined forces of the voluntary and involuntary muscles, we find—

Involuntary muscles =	54·10 lbs.
Voluntary muscles =	523·65 „
Total,	**577·75** lbs. av.

Thus we see that, on an emergency, somewhat more than a quarter of a ton pressure can be brought to bear upon a refractory child that refuses to come into the world in the usual manner.[a]

In order to determine, by actual experiment, the expulsive force of the abdominal muscles, I placed two men, of forty-eight and twenty-one years of age, respectively, lying on a table upon their backs, and put a disk measuring 1·87 inches diameter just over the navel; weights were placed upon this disk, and gradually increased until the extreme limit of weight that could be lifted with safety was reached; this limit was found to be, in both cases, 113 lbs. As the circle, whose diameter is 1·87 inches, has an area of 2·937 square inches, the pressure perpendicular to the abdominal wall produced by the action of the abdominal muscles is—

$$P = \frac{113}{2{\cdot}937} = 38{\cdot}47 \text{ lbs. per square inch.}$$

This force is somewhat greater than that calculated from the curvature of the belly and the coefficient of muscular contraction, already determined; viz., 32·93 lbs. per square inch. It should be remembered, however, that in my experiments on the abdominal muscles, I pushed the

[a] The preceding result will, no doubt, remind the curious and well-informed reader of the statement made by Mr. Shandy, on the authority of "Lithopædus Senonensis de partu difficili," that the force of the woman's efforts in strong labour pains is equal, upon an average, to the weight of 470 lbs. avoirdupois, acting perpendicularly upon the vertex of the head of the child.

trial of strength almost to the verge of producing slight *peritonitis*, as both the subjects of my experiment complained of sharp shooting pains in the abdominal walls for twenty-four hours after the experiments were made.

The foregoing theory of the muscular forces employed in parturition, leads us naturally to a caution as to the use of chloroform, and other anesthetics, which produce their effects, first upon the voluntary, and afterwards upon the involuntary muscles. When chloroform is used, in parturition, beyond the stage at which it produces simple drunkenness, and indifference to pain, it is positively injurious; for it destroys the action of the voluntary abdominal muscles, which constitute the chief part of the force employed in difficult labours. Hence, the muscular tissue of the uterus is left to overcome an obstacle beyond its strength, and without the aid that nature intended to be given by the abdominal muscles. The result is that labour ceases until consciousness returns, and enables the ill-treated woman to avail herself of the apparatus of abdominal muscles provided by nature for her use.

In these remarks I confine myself altogether to the physical aspects of the use of chloroform in labour; and forbear to express an opinion as to the moral effects likely to be produced by the exhibition of an intoxicating agent, administered merely for the luxurious purpose of deadening the pain of the exercise of a purely natural physiological function.

Ovarian Tumour, Complicating Labour. By George H. Kidd, F.R.C.S.I.; Obstetric Surgeon to the Coombe Lying-in Hospital, &c.

Ovarian tumours, as complicating labour, divide themselves into two classes, viz.: those lying low in the abdomen and obstructing the passage of the child through the pelvis, and those lying high in the abdomen and not interfering with the pelvis. Tumours occupying the first of these positions are, I believe, always small, and not infrequently solid, or semi-solid in structure, and they may be confounded with fibroid tumours of the uterus and tumours of the Fallopian tubes, or with abscesses and other tumours of the neighbouring parts, or growths from the bones of the pelvis itself. They generally lie in the recto-vaginal pouch of the peritoneum; but Cazeaux quotes a case as having been described by Jackson, in which the tumour lay behind the rectum, pushing it forward. In the absence, however, of all anatomical detail, it is difficult to understand how an ovarian tumour could get into this position. In a case of extra-uterine fetation recently under my observation, the fetus was encysted in the space between the rectum and uterus, and in many respects closely resembled an ovarian tumour. These tumours may be so small as to sink down completely into the cavity of the pelvis, or they may lie in and more or less completely obstruct the brim, and in either case form a very serious complication to labour.

It thus becomes a question of vital importance how labours complicated with tumours of this description are to be dealt with; and it is obvious that the tumour, if large enough to obstruct the passage, must either be shifted out of its place or lessened in size, or the child must be lessened in size, or removed through other than the natural passages.

In a paper of very great value, read by Dr. Playfair before the Obstetrical Society of London, and published in Vol. IX. of their Transactions, the details of fifty-seven cases, published by different authors, in which a tumour of the ovary was pushed down in front of the presenting part, and caused an obstruction to the passage of the child, are collected and tabulated, and the results of the treatment adopted compared. Out of the fifty-seven cases the labour was left to the natural powers in thirteen; and of these, seven of the mothers recovered and six died, being 46·1 per cent.; but in all these cases the tumour must either have been very small or very compressible, and it must have been subjected to an extreme degree of pressure and squeezing in the passing of the child, which may account for the very unfavourable results.

In nine cases the tumour was reduced in size by the evacuation of its contents through a puncture; all the mothers recovered and six of the children were saved.

In five of the cases the tumour was returned above the brim, and all the mothers recovered. In one of these the tumour was apparently solid, and pushed down before the head in such a manner that Cesarean section was contemplated; but Mr. Spencer Wells, who was consulted, succeeded in pushing the tumour up by introducing his whole hand into the vagina, after which delivery was accomplished without further difficulty.

In fifteen cases craniotomy was resorted to, in four of which the tumour had first been punctured; but in two it did not collapse, and in the other two it collapsed sufficiently, but craniotomy was performed because the child was known to be dead. In eight of the fifteen cases the mothers recovered, and in seven they died; but it is expressly noted in several of the cases that recovery was only imperfect. In five cases version was performed, in four of which the mothers died; and in two the forceps was used, and one of the mothers recovered.

It thus appears that no mode of delivery is at all safe in which the tumour is subjected to pressure, whether it be effected by the natural powers, by the forceps, craniotomy, or version; and that for safe delivery, the tumour must either be pushed aside or obliterated by the removal of its contents. The latter of these methods is, no doubt, the simplest and most easily performed, for it is only necessary to puncture it with a trochar through the vagina, and in the recorded cases it has been successful; yet it is not free from objections. In the first place, the tumour is often solid and cannot be reduced in size; and in the second place, we would all be glad to avoid the infliction of even the simplest

wound at the time of labour lest it should give rise to peritonitis or other injurious consequences.

The shifting of the tumour when it can be effected is unquestionably the best practice, though it may be difficult of accomplishment, and sometimes impossible, but still it should be attempted in all cases; and my chief object on the present occasion is to recommend the adoption of a method by which I have myself been very successful in raising pelvic tumours. This method has already been brought before this Society, and consists in the introduction into the rectum of one of Barnes' bags and the slow and forcible distension of it by air or water.

On the 8th of June, 1867, Dr. Ringland read before the Society a case of fibrous tumour of the uterus, in which he and I adopted this method, and succeeded in raising a tumour that had been pressing injuriously on the rectum, and since then I have used it in many cases; one of these was an unmarried lady that Dr. Denham asked me to see along with him, who had a fibrous tumour firmly fixed in the pelvis, and interfering with both the rectum and the bladder, and in which we raised the tumour out of the true into the false pelvis by this means, and effectually relieved the symptoms. In these instances, however, the patients were not pregnant, and the tumours were not ovarian.

On the 27th June, 1869, I attended a lady in labour with her third child, eleven years having elapsed since her previous one had been born. About a year and a-half before I had attended this lady for inflammation of the uterus and ulceration of the os, and during that attendance discovered an ovarian tumour, which lay so deep in the abdomen that she was not herself aware of its existence, though it was larger than the fetal head. When labour commenced I found this tumour occupying the brim of the pelvis, and pushing the os uteri upwards and forwards, so that it was with difficulty that it could be reached. As long as the membranes were unruptured I thought it better not to interfere, as there was no injurious pressure being exercised on the tumour, and I was not without hope that it might be moved out of the way, as in the case of the fibrous tumour so graphically described by Dr. Beatty in his recent volume. However, the membranes ruptured before the os was fully dilated, and instead of moving out of the way, the tumour became more and more tightly pressed into the pelvis. Having asked for assistance, Dr. Churchill saw the case with me. The tumour was now firmly fixed in the brim of the pelvis, it felt hard and solid, and seemed to be quite immoveable, no pressure that we could apply with the fingers making the slightest impression. Puncture of the tumour, and if this failed from its being solid, which seemed very probable, the Cesarian section were almost the only alternatives; but Dr. Churchill agreed with me as to the propriety of trying one of Barnes' bags in the rectum before taking any other step, so we placed the patient on her knees with her head and shoulders

as low as possible, very much in the position described by Thomas for the reduction of a prolapsed funis. The largest of Barnes' bags was now pressed into the rectum and gradually distended with air, while Dr. Churchill kept his finger on the tumour, and, to our very great satisfaction, felt it gradually move as the bag was distended. When it was full I gave it a sudden push, and at once Dr. Churchill said the tumour was gone. It disappeared with a jerk, and the os came fairly into the brim of the pelvis, so we emptied and removed the bag, and made the patient lie on her back till the head was fairly in the pelvis. In a few hours a fine healthy living child was born, and the mother recovered without a bad symptom, and has nursed her child and remained in apparently perfect health ever since.

I conclude this portion of my subject by laying it down as a rule, that in all cases of labour obstructed by a tumour in the pelvis, our first duty is to raise the tumour, if possible, out of the way; and when this cannot be done with the fingers placed in the vagina, it must be attempted through the rectum by some such means as those now described.

Ovarian tumours lying high up in the abdomen are more liable to be injuriously acted on by pregnancy than to interfere with labour, but they may give rise to post partum hemorrhage, or at least make it more difficult to control it when it does occur, as it is not easy to make pressure on the fundus to secure a good contraction as in the following case:—

Ovarian Dropsy and Pregnancy.—Mrs. T., aged twenty-nine, admitted to the Coombe Lying-in Hospital 29th January, 1856. Six years married. Far advanced in third pregnancy, last menstruation having been, she says, in the beginning of April. She now measures, when standing, 4 feet round the abdomen at the umbilicus; when lying on the back, 3 feet 9½ inches.

The abdomen has the appearance of being formed of two spheres, one large occupying the right side, front, and as far as a line drawn from the end of the ninth rib to an inch on the inside of the anterior superior spine of the ilium, on the left side. From this line another swelling extends to the left side, formed by the uterus, and appearing to be a portion of a smaller sphere. The large swelling is soft, even, and fluctuates; the surface is white, and marked by numerous blue veins. The small one regular, firm. It is here she feels the child's movements, and in the upper part of this the fetal heart may be heard, beating about 120 in the minute. The breasts are rather flaccid, but have the areola well developed.

The countenance has a rather worn expression, but is on the whole healthy. General health good, appetite good, bowels regular. The weight of the abdomen is the only thing complained of; to relieve this she wears a bandage.

History.—First child was born ten months after marriage; eleven months afterwards she began to suffer from cramps in the right iliac fossa, which were relieved by aperient medicines. A tumour was soon after found in this region, which, when first observed, was as large as an orange, moveable, and not painful to the touch. She now weaned her child, and in eleven weeks menstruation returned.

She has always suffered much pain in menstruation, chiefly in the right side, "a soreness," which was so great before and during the period that she could not stoop; it was relieved when the discharge ceased. The period occupied a week. For two days before it she suffers from leucorrhea, then the menses appear, with pain for a day or two; then there is an interval of a day without pain or discharge, both, however, returning and lasting a few days. This has been the case before marriage and ever since.

The second child was born two years and ten months after the first. At the commencement of this pregnancy, she says the tumour was the size of a small loaf. After the child was born she was as large as a woman at the full period of pregnancy. She nursed this child two months. Immediately after weaning it the menses returned. When this child was one and a half year old she again became pregnant, and she believes she is now at her full period.

During the interval between the second and third pregnancy she thinks the tumour did not increase in size.

She states that after the birth of the second child, a cyst, larger than the placenta, but not connected with it, containing "corrupted matter," was expelled immediately after the placenta.

24th March.—Labour set in to-day. The child was born as in ordinary circumstances, but after its birth there was considerable hemorrhage, and I was sent for. When I arrived, the placenta had been expelled, and some draining was going on. I administered ergot of rye; and it being impossible to feel the uterus externally, introduced my hand to remove any clots that might have formed, when the hemorrhage ceased. The child was a large healthy well-formed one.

April 17.—Convalescence has been good. She goes home to-day, to return after some time to have the dropsy treated. The abdomen now measures, when lying on her back, 3 feet 9½ inches, and when standing 3 feet 10½ inches. Child thriving well.

In this case we have the history of two pregnancies without any evil consequences from the tumour; and in the case of a lady I was called to see on the 26th May, 1864, the same thing occurred. When I was called in she had been delivered of a living child at full term, and the placenta was expelled naturally; but there was some alarm because

of the size of the abdomen. On examination I found the uterus well contracted, and an ovarian tumour lying above it as large as the uterus at the end of the ninth month of pregnancy. There was no hemorrhage, and she made a good recovery. After delivery the tumour continued to grow, but her general health remained good, and she again became pregnant and went the full period without inconvenience, and was delivered of a living child under my care. She proved pregnant a second time, and was delivered safely, and ultimately died of acute bronchitis.

We have thus, in two patients, the history of five pregnancies without serious inconvenience from the tumours, and so far as they go they show that we cannot accept without hesitation the dogmatic assertion of Dr. Barnes—"That nature would not tolerate the concurrent progress of these two conditions. Nature could hardly bear the simultaneous growth of two tumours like the pregnant uterus and an ovarian tumour. Something must give way." At a meeting of the London Obstetrical Society, in December last, Dr. Braxton Hicks related the particulars of ten cases, comprising eight pregnancies where pregnancy was associated with ovarian tumours, but were all delivered at the full time of living children without danger; but at the same meeting Spencer Wells related fifteen cases that he had seen, but though the results were much less favourable there seem to be still good grounds for hesitating to either induce premature labour, as recommended by Dr. Barnes, or to tap, as recommended by Wells, unless there be special reasons calling for interference.

On Congenital Tumour of the Head in a New-born Infant. By Dr. MacSwiney, Physician to Jervis-street Hospital.

I desire to call the attention of members of the Obstetrical Society, in a very brief communication, to a case which lately came under my notice, where a tumour of the scalp in a new-born infant existed at the time of birth, and presented some features of difficulty and obscurity as to its exact diagnosis. The history of the case was, shortly, as follows:—

Mrs. L. was confined of her eighth child on December 9, 1869. Her labour was natural and easy, it was speedily accomplished, and did not require instrumental aid. The infant was a large, well-nourished male, apparently very healthy. Nothing particular was noticed in the appearance of any part of the child's body of an abnormal character on the day of its birth. However, on visiting this lady the next day after the birth of the child, my attention was called by the nurse to something wrong about the infant's head. Upon examination I found there was a tumour situated upon the right side of the cranium, over the right parietal and part of the occipital bones, near their upper, or vertical, margin. This was covered by the scalp, which was of its natural colour, and well supplied with hair. It was round in outline, broader at the base than at

the apex, and had a dimension of $5\frac{1}{2} \times 4\frac{1}{2}$ inches. Its size was not increased when the child cried. It was full, firm, fluctuating, and elastic. Gently manipulating it, for the purpose of examination into its nature, did not appear to give any pain, and slight pressure did not in the least diminish its size. I made but slight pressure, as I was unwilling to ascertain experimentally, by any forcible compression, whether or not the contents of the tumour communicated with the cavity of the cranium. The cranial bone could not be felt at the bottom, but then, indeed, the tumour was so full that no degree of pressure which was justifiable with the fingers on its surface, could enable the examiner to reach to a level with the bones. Its base was clearly circumscribed by a thick ring of bony hardness, and the finger carried over this well marked edge or ridge, dipped into what appeared to be a bottomless cavity. I repeatedly tried to satisfy myself on the point, whether or not a communication existed between the external swelling and the meninges, with the result that I always thought nothing could be more complete than the resemblance presented upon each such examination to an absence of bone under the tumour, and, on the whole, I altogether inclined to the opinion that such osseous deficiency did really exist, notwithstanding that there was no pulsation in the tumour, nor were there any symptoms of compression present, facts in the case which, it is needless to say, told strongly against this opinion. When the infant was three weeks old, I asked my friend, Dr. T. M. Madden, to examine the tumour with me, and the following is a short extract from a note which he has written me:—"The edges were sharp, and it felt as though there was an actual deficiency of bone in the centre, and such was my opinion at the moment." I adopted the expectant plan with regard to this tumour, and no treatment whatsoever was had recourse to. For five weeks the tumour remained stationary, at least in so far as that not the slightest diminution in its size took place; indeed, in the opinion of the mother and nurse, it became larger, and I am inclined to think that this may have been the case. However this might be, at the expiration of these five weeks a process of absorption was suddenly set up, without any known cause, and in two days the tumour entirely disappeared, when the cranial bones were found perfect. The child has remained since then in complete good health, except that it suffers from an attack of moist eczema of the scalp.

I was at first at a loss to determine with what class of tumours to identify this swelling. I was familiar with the ordinary bloody tumour of the scalp, between the pericranium and tendon of the occipito-frontalis, sometimes met with in new-born children, and I, for a while, thought of referring it to this class. But the scalp tumour usually occurs in cases where the labour, mostly in a primipara, has been prolonged, or where instrumental delivery had been effected. No such exciting cause, however, existed in the present instance. The tumour, also, was more tense

and elastic than the scalp swelling usually is. Moreover, in ordinary *cephalematoma* the active vitality of the integuments of the head soon cause the absorption of the effused blood, whilst the lapse of many days found this tumour unaltered in size or appearance. I therefore, decided against its being the common scalp swelling. I next asked myself was it a *meningocele*, or was it an instance of that rare malformation, an *encephalocele*. Ordinarily the diagnosis of these congenital abnormalities is not difficult. Their situation generally in the middle line and at the occipital region, their pedunculated form, the symptoms their presence induces, the state of the infant's general health, the effect upon them of the crying or coughing of the infant—all these afford means sufficient, in most instances, to enable the physician to discriminate between these congenital protrusions of the cerebral contents, and other tumours on the scalps of new-born children. But then other situations are sometimes occupied by the "hernia cerebri congenita" (*Niemeyer*); in form it is often sessile, and cases, therefore, occasionally occur in which the diagnosis is not so easy. Thus, M. Giralde's (*Comptes rendus de la Societié de Biologie*), relates a case where a congenital dermoid cyst, or tumour of the scalp, was mistaken for a meningocele. Fluid was evacuated by an operation from this cyst, with a view to determine its nature, and was found to be identical in appearance with cerebro-spinal fluid. Its real nature, however, namely, that of a dermoid cyst, was subsequently made quite manifest. Again, I was at one time inclined to think that the case I am relating might have been a meningocele, whose communication with the skull had become obliterated. A preparation exists in the Museum of the St. George's Hospital, London, in which a spontaneous cure of a meningocele occurred.—(*Holmes*, p. 68). I also considered the question whether the fluid contents of this swelling was purulent, as it possibly might have been, and I concluded that it did not contain pus, as there had been no inflammation of the soft parts, and the feeling of fluctuation was unlike that communicated by an abscess.

Finally, I arrived at the opinion that this was an instance of a class of tumours whose occasional occurrence has been demonstrated, that it was, in fact, an example of an intra-uterine extravasation of blood between the pericranium and bone, the result of injury. There was, in fact, a circumstance, now to be related, in the history of the mother before the birth of the child, which much influenced me in coming to this conclusion, and which I submit to the judgment of the Society. Respecting the etiology of this class of tumours in general, I do not presume to advance any explanation, but with regard to the cause of the production of the swelling, in the particular case now under our consideration, I am able to present something definite by mentioning an accident which occurred to the lady about a month before parturition, which, I have no doubt, played an important part in causing the tumour to form. Mrs. L., wishing to adjust a window curtain, stood on a rather high chair. Whilst thus

engaged, one of the legs of the chair suddenly broke, and she fell to the floor, striking the lower part of the front of the abdomen heavily upon the edge of the back of the chair. She, of course, received a great shock and felt much hurt and frightened. She fully expected that premature labour would supervene. She lay upon a sofa for some time, and as nothing serious occurred, and the pain gradually subsided, she became reassured, and, after the lapse of a few days, thought no more about it, until asked by me if she knew any way of accounting for the swelling on the child's head, when she related to me the particulars of the accident which I have just brought under the notice of the Society. It is not for me to anticipate whether members will agree with me in ascribing the production of the tumour to this fall.—*Read 12th March*, 1870.

On a Remarkable Case of Complex Labour. By Thomas More Madden, M.R.I.A., L.K. & Q.C.P.I., M.R.C.S. Eng., Assistant-Physician to the Dublin Lying-in Hospital (Rotunda).

The following case being unparalleled, as far as I can ascertain, in the annals of midwifery, will probably be considered of interest by this Society.

February 9th, 1870.—I was sent for, at 7. 30 p.m., to visit J. S., of 7, Moore-street, a patient attended by the pupils from the extern maternity of this hospital. She had been in labour, and the liquor amnii had been evacuated for twenty-four hours. The pupil in charge of the case (Mr. E. B. Roche), to whose notes I am largely indebted, was not sent for till an hour previously, *i.e.*, 6·30 p.m., when on examining her he found about three inches of the cord prolapsed, and two feet, with a rounded tumour behind, then presenting. He, not unnaturally, concluded this was a breech; and as the cord was pulsating strongly endeavoured to return the prolapsed funis. In doing this he discovered, to his great surprise, that the rounded tumour he felt was not the breech but the head tightly jammed down through the brim by the closely contracted uterus. He then made a decided attempt to push the feet and cord above the head, but so tightly were they caught between the head and the pelvis that he could make no impression on them. He now sent for assistance; and endeavoured in the meantime to keep the pressure off the funis. On my arrival I found Mr. Roche's account of the presentation perfectly accurate. "The feet were presenting at the posterior part of the vagina, the heels to the sacrum, the toes pointing directly forwards. The feet could be circumscribed and followed up to the lower part of the legs, where they were tightly caught between the head and the promontory of the sacrum. There was a loop of the funis, about three inches, prolapsed between the legs, and pulsating strongly in the intervals of the pains. The head lay in the second position, or rather more transverse, and was jammed down upon the feet and cord;" the pulsations in the latter now ceasing during each pain. The uterus being very tightly contracted on the fetus, and

the liquor amnii having been evacuated for 24 hours, and knowing from experience on other occasions the great difficulty of performing version under these circumstances, I proceeded to apply the long forceps, in the hope of bringing down the head past the feet; they however failed to make any impression, although I tried them in the occipito frontal as well as in the transverse diameters, and as they slipped in both, after several attempts, I abandoned them.

The funis was still pulsating, but less strongly than when I first arrived. She was now put under the influence of chloroform, by Mr. A. C. Roberts, and this had the effect of so far relaxing the parts that I was enabled, though with considerable difficulty, to draw down one foot; this I could not do before the chloroform was given. The foot was then secured by the attending pupil whilst I reintroduced my hand, and, using some force, succeeded in pushing back the head through the brim of the pelvis and into the uterus, at the same time making forcible traction on the foot. After these efforts had been continued for some time without apparent effect, the fetus suddenly rotated, the foot came down, the head receded, and the delivery was now easily accomplished. The child was born alive, though apparently still-born, and was resuscitated by the diligent application of the ordinary measures. There was a deep indentation on the right parietal bone where the head had been compressed between the feet and the pelvis. She made a good recovery, and some days after I learned that mother and child were doing well.

There appear to me to be two points of interest in the foregoing case; the first is the extreme rarity of a compound presentation such as this; the second is the length of time which had elapsed between the rupture of the membranes and the birth of a living child in this case of prolapsed funis.

The extreme rarity of a case such as that just described may be inferred from the fact that in none of the statistical reports of the Rotunda Dublin Lying-in Hospital, which, conjointly, afford the largest, and perhaps the most accurate, mass of obstetric statistics in existence, is there any mention of a similar case of complex labour. Thus Dr. Collins, who gives an account of 16,654 deliveries, does not allude to such a complication. Neither do Drs. Hardy and M'Clintock in their report of 6,634 labour cases; nor is there any similar case in Drs. Johnston and Sinclair's account of 13,748 deliveries. Thus in 37,036 cases which are reported on by the Masters of the Dublin Lying-in Hospital, no case such as that now laid before the Obstetrical Society occurred. Dr. Churchill in the last edition of his *Theory and Practice of Midwifery*, although he devotes a section to the consideration of "Compound Presentations," does not refer to any case similar to that which form the subject of this paper. Nor is there such a case described in Dr. Ramsbotham's *Obstetric Medicine and Surgery*.

Mauriceau is the only writer that I know of who records a case nearly analogous, and his observations on this, as well as other cases of compound presentations which he describes, although made some two hundred years ago, are so judicious, and so well worthy of being borne in mind, that I shall make no apology for translating two of his cases which appear to me to bear on that which has just been laid before the Society :—

" Observation XLV.—De l'accouchement d'eme femme dont l'enfant presentoit le coste de la teste avec un pied et chute du cordon de l'ombilic.

" On the 29th of October, 1671, I delivered a woman of a large child, which presented the side of the head with a foot and prolapse of the funis as far as the external orifice of the vagina, in which cord evident pulsation was to be felt, which made me know that the child was still alive, although it had been nearly two hours in this unfortunate situation before the midwife who assisted the woman sent for me to aid her. But what is worthy of note in this case is that it was the foot of the child that presented with the head, which saved this infant from perishing, by preserving the umbilical cord from entire compression by the round head of the child, as must have occurred if the cord had presented by itself together with the head, which compression, by interrupting the circulation of the blood, that should there take place freely, must of necessity have caused the death of the child, which I, however, delivered alive by drawing down the feet, after having a little pushed back the head with the umbilical cord, in order to make the extraction more easily." [a]

" Observation CXLV.—On the 17th of October, 1675, I delivered a woman of a very large living child which had the feet and hands strongly engaged in the passage with the head when I was sent for to her assistance. Her midwife had previously endeavoured to draw down the child by a foot, but instead of doing so had but more firmly empacted the head in the passage for want of having returned it within the pelvis, as well as the hands which also presented. Before drawing down by the feet, as I did in her presence, making her understand that in these operations the good judgment of he who works is still more necessary than strength of body, which she said she had not, in order to be able to affect those laborious deliveries." [a]

With regard to the treatment adopted in the foregoing case I shall make a very brief allusion. I have been told that considering the length of time the liquor amnii had been evacuated, and the tightness with which the uterus was contracted on the fetal body, that I was not

[a] Observations sur la Grossesse et l'Accouchement des Femmes, et sur leurs Maladies. Par François Mauriceau, &c., p. 29. Paris, 1695.

justified, having a due regard to the safety of the mother, in returning the head into the uterine cavity, and performing version; and a strong hint was thrown out that I should have used the perforator under such circumstances. Other eminent obstetricians, whose opinions on any question connected with midwifery are entitled to great weight, found some fault with my practice in having first attempted to apply the forceps in this case, and not having at once brought down the feet, and I much regret being obliged to differ from their views, as I must do on this question, for though "*Amicus Plato, amicus Socrates, sed magis amica veritas.*" It is a great satisfaction to me, however, that my practice meets with the full approval which has been expressed by the accoucheur who is officially the head of our department of the profession in this country, the President of this Society, and Master of the Lying-in Hospital. I still think that what I did was right, and had I such a presentation to deal with again I should again act as I did in the foregoing instance, and should be well satisfied if I again saved the lives of both mother and child under such circumstances. I shall not stop to discuss the hint thrown out as to the necessity of perforation. My views on this point are too clear to admit of a shadow of a doubt upon it. I do not think it morally right to destroy any living human being. Most assuredly then I do not regret not having destroyed a child which, with safety to its mother, I was enabled to deliver alive.

With regard to the opinion that I should not have attempted the forceps in this case, I would only remind the Society of the fact that the head of the child having passed through the brim was lying in the pelvic cavity, within easy reach of the forceps, and in the second position; that the feet were far up at the brim of the pelvis and that the waters had been evacuated twenty-four hours before I was sent for. It is obvious that the forceps if successful would, under these circumstances, be applied with greater safety to the mother than generally attends the operation of turning. As to the rule laid down by some writers of never applying the forceps in funis presentations lest the cord should be compressed thereby, I must say that I attach no practical importance to it. No person should ever attempt to apply the forceps who does not possess sufficient dexterity in their use to avoid the possibility of so untoward an accident. The question of turning or applying the forceps in funis presentations must be decided in every instance by the circumstances of each particular case, and if the head be low down in the pelvic cavity I think the forceps should be tried before resorting to version. That the funis was not compressed by my application of the forceps is proved by the fact that the child was born alive after their use.

I may also mention, that in the same week in which I attended the patient of whose labour I have just given the history, no less than four cases of funis presentation came under my observation; in one of these I

delivered by the long forceps, in the others I performed version, and in all of them the child was born alive. I need not remind this Society that in cases of funis presentation the statistics of our hospital and other similar institutions show that more than one-half of the total number of children delivered are born dead in consequence of pressure on the cord during labour. Therefore, if the foregoing case occurred again I should again attempt to use the forceps before turning; but, before doing so, I should follow the recommendation of Dr. Johnston and Dr. Denham, and put my patient under the influence of chloroform, by which, perhaps, the parts might be sufficiently relaxed to enable the extraction with the forceps. This failing, version must be resorted to; and the operation in question certainly amounted to something more than version. It was not a mere extraction of the child by drawing down the feet, for the head had to be moved out of the cavity of the pelvis and passed back through the brim into the uterus, whilst powerful traction being exerted by myself and my assistant on the feet, the body of the child was made to rotate. The difficulty of doing this in a case where the uterus was so long, and so powerfully contracted around the child's body, must be obvious to any obstetrician; and none but one who had his hand in a uterus firmly contracted on the child can well judge of the amount of force which may be employed, and suffice to overcome the strength of the contraction, without running the danger of thereby rupturing the uterus. It was very satisfactory to find that in this case the mother did as well as the child, and convalesced rapidly.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

Dr. M'Clintock, President.

Aortic Aneurism.—Dr. William Moore said, the morbid specimen he now exibited was taken from the body of a man, 31 years of age, who had been admitted to Sir Patrick Dun's Hospital on the 31st December. About three years previously he was engaged in a very laborious occupation. He was employed in coaling the Cunard steam ships in Liverpool, and had often worked continuously for 24 hours, during which time 120 or 130 tons of coal would be put into the ship. On one occasion he fell into the hold of one of these ships, about 20 feet. A short time afterwards he for the first time complained of pain in the back; on resuming work he found that he had this incessant pain in the back. He came over to Dublin and placed himself under the care of Mr. Tufnell, and at that time he complained also of palpitation in the epigastrium. Under treatment he got rid of this palpitation, and resumed his work, but he was

obliged to give it up and came back to Dublin, where he was again under the care of Mr. Tufnell for eight months. He then returned to Liverpool. When he came under Dr. Moore's care he was very pale, and complained much of pain in his back. He suffered from pain in the region between the ensiform cartilage and the umbilicus extending up under the left floating ribs. He passed his urine freely and naturally; it was of normal specific gravity, and contained no albumen. There was no jaundice, nor any œdema of the legs or feet, or ascites. The pulsation was feeble in both iliac arteries; there was visible pulsation in the radial arteries. The pulse was 84. On making a more careful examination, dulness was found over the pericardial region, and a peculiar click, an intensified throb, so to speak, but no bruit. There was visible pulsation below the ensiform cartilage, and dulness on percussion. On examining the same region between the ensiform cartilage and the umbilicus with the stethoscope when he was recumbent, a distinct bruit was audible. If placed in an upright position the bruit became inaudible. On being turned on his face, at the level of the last dorsal vertebræ a well marked bruit could be heard, particularly at the left of the spine, which became intensified, until the fourth lumbar vertebra was reached. When placed on his hands and knees he did not lose the pulsation in front. Matters went on thus for some time, and the man became quite cheerful, and every day he got up and went about the wards. On the day he died, he was going about as usual, when he called out suddenly, "Send for the doctor," but before the resident pupil could come to him he was dead.

The *post mortem* examination revealed general hypertrophy of the heart. There was frothy mucus in the larger tubes, and a rather congested condition of the lungs. Upon examining the abdominal aorta, an aneurism was found which was as large as an orange. It was given off immediately at the celiac axis. It had obliterated the renal artery, and the right kidney was reduced to an ounce in weight.

The case was interesting in many points of view. Had they contented themselves with examining the patient over the front of the abdomen they would have perceived nothing which could not be accounted for by the existence of a tumour, a psoas, or hepatic abscess; the indications to be observed in front were common to those tumours that pressed on the vessels; but if they placed the man on his hands and knees, the tumour would fall forward, and the bruit would disappear. In the majority of such cases there would be a total absence of bruit when the patient was placed on his hands and knees. In this case, owing to the persistence of the bruit, in the prone position, coupled with the other symptoms, they had no difficulty in diagnosing it as a case of aneurism. He might be asked was this persistence of bruit pathnogmonic of aneurism? It was not; but its being confined to two or three inches along the spine from the

last dorsal to the fourth lumbar vertebræ, prevented it being confounded with spanemic or obstructive aortic murmurs, which would have been continuous from the nape of the neck downwards.—*February* 6, 1869.

Cerebro-spinal Meningitis.—Dr. Grimshaw exhibited the brain and spinal cord of a female, aged twenty-one, who had died in Steevens' Hospital, under the care of Dr. Swan. She had been a fortnight ill before she was admitted to hospital, on the 30th of January. She had some nausea, headache, and general uneasiness, together with some slight cerebral symptoms. The only peculiarity about her was that she got up once or twice during the night and walked about, in consequence of great pain in the head. These attacks were sudden. Some days before admission to hospital she got diarrhea and vomiting, the diarrhea being of such a nature as to make one suspect that she had typhoid fever. On admission to hospital she was vomiting, and had still diarrhea. The pain in the head of which she complained was very considerable; she had pains in the shoulders also, and in the back of the neck. In two days after admission, on the 2nd of February, she had strabismus of both eyes; the pulse was 96, the temperature 101·2. During this day she became quite comatose, and towards evening muttering delirium. Her pulse was then 120. He (Dr. Grimshaw) saw her on the following morning. He found her with a quick pulse of a peculiar jerking character; the head slightly drawn back; no stiffness of the limbs; quite insensible; the vomiting of the peculiar green colour seen in these cases. He came to the conclusion that the case was one of meningitis, although the symptoms were not well marked. The vomiting had ceased, but the diarrhea still continued. On the following day she was worse, and manifested another symptom of meningitis; hyperesthesia well marked on the right hip, and along the right thigh. On the 5th she was still worse, and died on the next evening. The temperature before death was 99·6; the pulse was imperceptible. She had not passed water during the two days preceding her death, except with the assistance of a catheter. No spots of any kind appeared on the body. The case was remarkable because of the slowness of the invasion of the disease.

The *post mortem* examination was made that day (6th February). The rigor mortis was well marked; the whole of the body was covered with dark purple patches, none of which, as he had already said, appeared before death. On making an examination of the brain and spinal cord, a large quantity of the usual dark fluid blood exuded from the vessels. The cord presented a considerable amount of inflammation. There were many patches of lymphy effusion. The cord was rather firmer than usual; the upper part of the cord had not the pathological characters so well marked as usual in these cases; they were more marked in the brain than in the cord. The arachnoid over the base of the brain was opaque,

and at one point a mass of greenish yellow deposit was present. The arachnoid at the upper surface of the cerebellum was opaque, and there was a considerable amount of deposit; the whole of the sub-arachnoid space was filled with fluid; there was a great deal of adhesion between the hemispheres. The intestines presented no pathological change indicative of typhoid fever.—*February* 6, 1869.

Ovarian Tumour.—Dr. Bennett exhibited an ovarian cyst which he had removed from a body in the dissecting-room of Trinity College. He had no knowledge of the history of the case during life, and exhibited the tumour to the Society on account of the remarkable chemical peculiarity of its contents, and some points of anatomical interest in its structure.

The tumour was very prominent in the abdomen of the subject, and projected much to the right side, and two or three irregular projections could be felt to the left side, and in the pelvic region. On opening the abdomen the tumour was found to be entirely adherent in front to the abdominal wall, while it was free from connexion with any of the abdominal viscera. On detaching it from the abdominal wall it was easily raised up, and was found to have a very long pedicle formed of the broad ligament of the uterus. The pedicle was remarkably twisted, apparently by a complete rotation of the tumour during its formation. This twisting had nearly divided the Fallopian tube, and must have caused much compression, for a time at least, on the vessels of the pedicle. The Fallopian tube, being attached to the upper part of the tumour, was greatly elongated, being fourteen inches long. It was attached to the tumour by a distinct process of the broad ligament in its entire length, and was pervious throughout.

The tumour, with its pedicle, when removed from the body, weighed twenty-five pounds. It was a multilocular cyst of the second variety described by Hodgkin. A great quantity of fluid was drawn off from the principal cyst, which was divided in many places by septa, the remains of cysts which had apparently burst into the principal cavity. The fluid was dark, of the colour of coffee grounds, and had a distinct bilious odour. The smaller cysts contained a dark fluid, not as turbid as that of the large cavity, but in all possessing the same odour as it. On allowing the fluid to rest, there were seen abundance of crystals of cholesterine. The odour of the fluid was so striking that Dr. Bennett submitted it to chemical examination in the laboratory, with a view to determine the presence of the biliary acids. The fluid was treated so as to coagulate the albuminous matter, and filtered. The clear fluid so obtained, submitted to Pettenkofer's test, gave the most unequivocal reaction, showing the presence of the biliary acids. The experiment was repeated on the contents of several of the smaller cysts, under Dr. Apjohn's personal direction, with the same result.

The fluid had, in addition to the chemical reaction and odour, the bitter taste of bile. Dr. Bennett considered the case worthy of being brought before the society, as he was not aware that any case had been recorded of the presence of the biliary acids in the fluid of ovarian cysts. There was no trace of connexion in this case between the tumour and the biliary passages, or liver or intestines, nor was there any trace of jaundice present. The twisting of the pedicle, and the great length of the Fallopian tube, were also worthy of notice.—*February* 13, 1869.

Gall-stone.—Dr. MacSwiney said the case he had to bring under their notice illustrated one of the dangerous effects, ending with death, which the action of gall-stones in the human gall bladder was capable of producing. The records of that Society and the pathological museums of that and other cities exhibited abundant evidence and examples of various, serious, and sometimes fatal results, caused by gall-stones. In this way—pyemic abscesses, inflammation of the biliary passages, peritonitis, fistulæ and other grave, and often mortal lesions had been produced. The case he now presented was an example of a fatal result from the action of a gall-stone in its transit from the gall bladder. The patient was aged 50. She entered Jervis-street Hospital on that day week. She stated that she had been in the enjoyment of remarkably good health up to the period when her last illness commenced. The only drawback to her general good health was that she suffered occasionally from what she called windy colic, which submitted to homely treatment, such as warm applications to the abdomen and a warm drink. On Thursday week last she said she was perfectly well in the morning, eat her breakfast heartily, and very shortly after it she was attacked with a violent pain, which she referred to the right side under the ribs. This pain was succeeded very soon by severe vomiting, and she stated she continued in a condition of great suffering, vomiting everything which she took during the whole of Thursday, the whole of Friday, and up to the afternoon of Saturday, having consulted no doctor, and relying on ordinary household remedies, a mustard plaster, &c. Dr. MacSwiney saw her on Saturday evening after her admission. Her condition was as follows:—She was slightly jaundiced. Her face, hands, and feet were perfectly livid, and quite cold, and she was bathed with a cold sweat. There was no pulse to be detected at the wrist, and when he had to determine the circulation he had to apply the stethoscope over the region of the heart. Her tongue was coated with a yellowish white fur. Her thirst was intense. She vomited everything that was given to her, cold water, iced water, brandy and water, chicken broth. The state of the abdomen was this. It was tumid in a remarkable degree. It admitted, speaking generally, of palpation, or compression with the fingers, without any inconvenience or complaint. It was tympanitic in a remarkable degree and a space about two or three square inches at the right side

immediately below the ribs, and extending into the epigastric region was hard, dull on percussion, intensely painful, and this pain was greatly aggravated by movement. In fact the character of her pain was remarkable. It was of a threefold nature, like that which is so generally regarded as the pain that characterizes biliary colic, it consisted of a persistent constant uneasiness, added to which were paroxysmal attacks of intense suffering, intermitted by periods of repose, and in addition to this there was the fact that the slightest movement aggravated her sufferings exceedingly It was quite plain that there was a local circumscribed peritonitis, and it was assumed that there was some obstruction to the peristaltic action, whether of a paralytic or of a mechanical nature he could not say. He suggested that she might be passing gall stones. She continued to gradually get worse, and died on Monday morning, 36 hours after entering the hospital, retaining her intellectual faculties to the last.

A *post mortem* examination was made. She was without exception the fattest corpse he ever saw opened, and that had a direct bearing on the nature of her malady. Upon opening the abdomen the signs of recent peritonitis in a circumscribed area were very easily discerned. The upper surface of the liver was attached by recent lymph to the surrounding parts; the great omentum which projected outward was attached very slightly to the parts with which it was in immediate contact. The lymph was remarkable. It was green, the colour of bile; it was obvious there was a considerable quantity of bile poured out which had been mixed with the lymph. The adhesions were easily broken up and were evidently quite recent. In the stomach there was a quantity of greenish, mucous looking fluid. There was no solid matter of any kind that he could determine. It was found that the stomach was adherent by very recent adhesions to the gall bladder, at the place where the gall ducts unite to form the common duct. Upon detaching these adhesions it was found that there was a large pouch at the junction of these ducts which was full of bile; the gall-bladder contained a number of polyhedral gall-stones without a drop of bile. There was corresponding to the adhesion to the gall duct an opening into the duodenum which admitted of the handle of a knife. Tracing down the common duct to its opening in the duodenum it was found to be considerably enlarged in calibre, and at its extremity just where it opened into the duodenum there was found firmly imbedded a large gall-stone. This could be recognized on the internal surface of the intestine and felt with a probe, but he was unable to pass the probe from the intestine into the gall-duct, and was unable to say whether this passage was pervious or not. The condition of the parts was this:—The gall-bladder was full of gall-stones and empty of bile; there was an adhesion between the duodenum and the lower end of the gall-bladder; there was perforation there; there was an

effusion of bile which was recognized in the recently effused lymph, by its green colour, and finally there was a large gall-stone embedded at the end of the common duct. The sequence of pathological events was doubtful, but he supposed that the large gall-stone which was found at the end of the common duct must have been the initial object in the changes that had taken place. Its position might have been taken up by it for a considerable period antecedently to death, and the bile may have trickled in small quantities through it into the duodenum, or it may have completely closed the duct, producing an accumulation of bile in the gall-bladder, irritation of the biliary passages, adhesion of the duct to the duodenum, and effusion of bile through the ulcerated opening.—*Feb.* 13, 1869.

Intra-thoracic Malignant Tumour simulating Aneurism.—Dr. Little said that the interest of the specimen he now exhibited depended altogether on its clinical history. In the early part of October a man came under his care in the Adelaide Hospital, aged fifty-four. He was a fine specimen of the English mechanic, and had been employed in India, Egypt, and elsewhere abroad. He had been three years in Dublin. His health had been good until about a year before he sought admission into the hospital. He then spat some blood. After this he lost flesh, and felt unable for much exertion, but had no marked symptoms of illness. Six months before admission to hospital he began to suffer from difficulty of swallowing. When he eat, the morsel, he said, stuck behind the breast bone low down. After this had continued for some time another symptom arose. He lost his voice, and became subject to a constant tickling cough. On admission to hospital he was somewhat emaciated and weak. The marked symptoms were slight difficulty of swallowing, aphonia, constant tickling cough, which brought up mucus tinged with blood, wandering pains about the chest, but particularly a pain on the left side, between the spine and the scapula. On a physical examination of the chest there was but one departure from health discovered, diminution of the respiratory murmur in the left lung. He ventured to make the diagnosis of an aneurism of the aorta involving the left angle and descending portion of the arch. In a fortnight the man's friends took him out of the hospital. He (Dr. Little) kept him under observation, and went to see him now and then. He gradually became worse, worn out by the incessant cough, which prevented him taking food or sleeping, and died in the beginning of January. Soon, however, in the progress of the case, there were signs which made him doubt the accuracy of his original diagnosis. There came dulness on percussion in the front of the chest, and that dulness spread to the left of the sternum, not to the right. Now they knew when an aneurismal tumour was making its way to the front of the chest it generally went to the right of the sternum and not to the left.

There was no impulse, no arterial sound, over the dull region. There was another circumstance which made him doubt that it was an aneurism. The man became steadily worse. Now, in an aneurism, they knew that the course from the commencement of the disease to the death of the patient was intermittent: the patient would get better, then worse, then improve, then get bad again. But there was a third circumstance which made him doubt that the man had an aneurism, and he mentioned it because of its diagnostic significance—the application of leeches to the chest gave no relief. They knew that no matter on what structure the pressure of an aneurism was exerted, whether on the œsophagus, a bronchial tube, or the wall of the chest, the application of leeches gave at least temporary relief.

On opening the chest he found the man had no aneurism, but a large encephaloid growth, a portion of which he now exhibited, and which occupied the whole of the anterior and posterior mediastinum, and which he believed had its origin in the posterior mediastinum. At a late meeting Professor Moore showed a specimen of aneurism of the aorta, the existence of which had not been indicated during life by any of the ordinary symptoms; and, as a contrast, this case was interesting as showing that they might have all the symptoms that pointed to aneurism—difficulty of swallowing, aphonia, lessened respiratory murmur in one lung, constant cough, bringing up mucus tinged with blood, and yet that the disease present might not be aneurism. The fact that the leeching of the chest did not give any relief was the first thing that made him suspect there was no aneurism. Since, several years ago, he had heard Dr. Stokes tell of a patient with thoracic aneurism, who came for many months to the Meath Hospital to have leeches applied on account of the invariable relief they afforded, he (Dr. Little) had been greatly struck with the constancy with which the withdrawing of a few drachms of blood, in cases of aneurism, gave at least transient relief to the pressure symptoms, whether dysphagia, dyspnea, or pain, an effect so constant that its non-occurrence in the case he now detailed made him doubt the diagnosis, notwithstanding how strikingly suggestive of aneurism were the symptoms.—*February* 13, 1869.

Disease of the Mitral Valves.—Dr. Hayden submitted to the Society two hearts illustrating the subject of constriction of the mitral orifice, which, in Dublin at least, was a comparatively new subject, and one upon which there was still a great difference of opinion. These specimens were somewhat old, but he thought the subject was new and interesting. The first of these hearts was taken from a boy ten years of age, who was admitted into the Mater Misericordiæ Hospital under his care on the 3rd of February, 1868. He learned that five years

previously the boy had scarlatina, and spat blood. When he saw him for the first time the pulse was scarcely perceptible. He had quick respiration, cough and mucous expectoration. He spat blood about once every three weeks, the hemoptysis continuing three days, and always attended with considerable alleviation of his symptoms. He remained under treatment a short time, and left the hospital at his own request, much relieved, but without any alteration in the physical signs. The prominent physical sign was a pre-systolic murmur at the apex of the heart, and from this he had no hesitation in diagnosing constriction of the mitral orifice. The patient presented himself again at the hospital in the end of February, and also on the 23rd of March. There was no change, except that on the last occasion he (Dr. Hayden) discovered at the apex of the heart both a systolic and a pre-systolic murmur. He continued to spit blood in large quantities, and was re-admitted on the 22nd of June. He was then unconscious, his face puffed and pallid, lips livid, no pulse at the wrist, the pre-systolic murmur less prominent, but still distinct. He died in a few days, and, on a *post mortem* examination, the following was found to be the condition of the heart :— There was very little alteration in the right cavities. The right ventricle was a little dilated, and the auriculo-ventricular orifice considerably so, three fingers passing through it easily. The left auricle was considerably dilated and thickened. The mitral orifice was much reduced in size, so much so that it barely admitted the barrel of a quill. The left ventricle was in a normal state, neither dilated nor thickened. The pericardium was found firmly adherent to the heart.

The second case was that of a female, aged thirty, who came up from the west of Ireland to be treated for jaundice. She complained of no other illness whatever. The jaundice appeared for the first time four months previously, and one month after her first confinement. There was no complaint of pain, nor anything to attract special attention to the chest. The pulse was 84, the skin of a deep yellow tint. She passed urine in large quantities. There was scarcely any tenderness in the hypochondriac region. In a hurried examination of the heart he thought he detected a systolic murmur. The symptoms, however, were not of a kind to attract any further attention to the heart, and he did not repeat his examination till the 1st of July. She had then undergone considerable improvement. The jaundice was less decided. She mentioned, however, that she had a "fluttering about the heart," and he made a careful examination of the organ in the corridor of the hospital, where she was taking exercise at the time, and at once declared the case to be one of constriction of the mitral orifice, in the absence of all symptoms and signs of organic disease of the heart, save the pre-systolic murmur.

On the 3rd of August she was struck down by typhus fever. He saw

her on that morning; the pulse was then 120. The murmur at the apex of the heart was still more decided in character. On his return from Oxford on the 11th August he found her body covered with dark spots, interspersed with petechiæ. She died on the following day. There was about a pint of fluid found in the pericardium. The heart weighed eleven ounces and a half. The right cavities were in the condition already mentioned when speaking of the first case. The right auriculo-ventricular orifice was dilated as in that case, but the right ventricle had undergone no change either as to the thickness of its walls or the size of the cavity. The left auricle, as was invariably the case, was enlarged and thickened. The mitral orifice was constricted, admitting easily the point of the index finger, and converted into a button-hole slit by partial agglutination of the valves. The chordæ tendineæ were shortened and thickened. The left ventricle had undergone no change, being of the ordinary size and thickness.

He should not further dwell on these cases, except to say that they afforded a favourable illustration not only of the practicability of diagnosing constriction of the mitral orifice, but likewise of its comparative facility. In the latter of the two cases there was no evidence whatever of disease of the heart, with the exception of a single sign, yet from this single sign he had no hesitation in pronouncing, with perfect confidence, the diagnosis of mitral constriction.

One or two features were worthy of notice. First, the suppression of the pulse in the greater number of these cases, and the remarkable apathy in regard to suffering: the patient's walking about the ward, with no pulse perceptible at the wrist, and declaring, in answer to inquiries concerning their health, that they were better; and lastly, the frequent recurrence of hemoptysis, which was a symptom to be met with in every one of these cases.—*February* 20, 1869.

Cancer of the Liver.—Dr. Cryan said that James W—, a wool-packer, aged 52, was admitted into St. Vincent's Hospital, under his care, on the 19th of December, 1868. For the past 6 or 8 years the patient has been in the habit of drinking from two to three quarts of porter daily, but he, nevertheless, usually enjoyed good health up to seven months before the period of his admission into hospital; he then began to complain of want of appetite, occasional nausea, and gradual loss of flesh and strength. On admission he looked pale and anemic, and was much depressed in spirits; he was greatly emaciated, and complained of loss of appetite, nausea, flatulence, and extreme debility; there was slight tenderness over the epigastric region, but no pain there, or over the liver; he had never vomited blood, or passed any from the bowels, nor had he been affected with syphilis, or disease of the bones; the liver could be felt projecting fully an inch and a half below the right costal arch; it was

hard and smooth, and slightly tender on palpation; the hepatic dulness, in the right mammary line, measured $6\frac{1}{2}$ inches; there was no evidence of splenic enlargement; the peritoneum contained a small amount of fluid; the girth of the abdomen, at the umbilicus, measured 34 inches; the superficial veins of the abdomen were not enlarged. The urine was scanty—about 20 ounces—sp. grav. 1032, acid, dark like porter, and threw down a copious deposit of lithates; it contained some bile pigment, but no bile-acids, and no albumen; tongue moist and covered with a thin white fur; bowels free; motions contained plenty of bile, and were not offensive. The respiratory signs were normal.

The impulse of the heart was very weak, the *first* sound short and clear, resembling the second in these characters; the area of cardiac dulness was slightly diminished; pulse, at the wrist, was small, weak, but regular, 80 in the minute; skin hot and dry, but neither it, or the conjunctivæ presented any jaundiced tinge.

Dr. Cryan said that when he first saw the case, he had some little doubt as to the nature of the hepatic enlargement—the intemperate habits of the patient, the firm, resistent, though smooth, liver felt below the right costal arch, the dyspeptic symptoms and the ascites, seemed to suggest that it might be due to cirrhosis in its first stage, in which, before the process of contraction sets in, we occasionally find the organ slightly enlarged, and firm and smooth; but, against this view were the absence of splenic tumour, of any enlargement of the superficial abdominal veins, or of gastric, or intestinal hemorrhage, and the marked cachexia, and rather rapid course of the disease; again, the smooth, painless enlargement of the organ might arise from waxy liver, but in waxy enlargement the progress of the disease is slow, and there is usually, enlargement of the spleen, albuminuria, with marked increase in the quantity of urine, and a history of constitutional syphilis, or of chronic disease of the bones,—all of which were absent in the case before us. That he (Dr. Cryan) therefore, thought it probable, notwithstanding the absence of pain, and the inability to feel any tubera projecting from its surface, the enlargement was due to cancer of the liver.

On the 30th of December—ten days after the patient's admission into hospital—two or three round firm nodules, each about the size of a marble, were distinctly felt on the anterior surface of the left lobe; the entire liver had, meantime, increased much in size; the emaciation continued to progress; the patient passed sleepless nights—though it is worthy of record that *he never complained of pain*—and the cachexia was more pronounced, so that there could be no longer any doubt as to the cancerous nature of the malady.

On the first of January, 1869, he became slightly jaundiced; the stools still contained an abundance of bile—the jaundice persisted till his death. On the 29th of January—ten days before the patient's death—the *right*

lower extremity became anasarcous, the swelling extended gradually up to the crest of the ilium; there was no tenderness over the veins of the limb, nor did the patient complain of pain; the skin was pale, and the temperature a few degrees below the normal standard; pulsation could be felt in the dorsal artery of the foot. From this date, until he died of exhaustion, on the 8th February, the œdema of the lower limb slowly increased.

Post mortem examination, 14 hours after death, exhibited the following appearances:—The liver was enlarged, weighing 5lbs. 4oz., and was thickly studded on both surfaces, and throughout its interior, with round whitish tubercles of brain-like consistence, and varying in size from a pea to a chesnut, but few of these tubercles presented the usual cup-like appearance on the free surface, or showed any central softening; the hepatic tissue surrounding them was for the most part of a yellowish brown colour, but at some points, where the tubera were of large size, the liver substance was of a greenish hue, and seemed condensed by their pressure; three or four large nodules projected from the anterior surface of the left lobe of the liver, close to its thin edge. On a microscopic examination of the cream-like fluid obtained by scraping a section of the tubercles, it was found to be made up of large nucleated, round, oval, and fusiform cells, ranging from the $\frac{1}{500}$ to $\frac{1}{1000}$ of an inch in diameter, with a nucleus about one-third the size of the cell, and surrounded with much fine granular matter, which resisted the action of ether, and was not of a fatty nature; some of these cells contained two nuclei; the hepatic lobules were not infiltrated with cancer, but many of the hepatic cells were loaded with oil, and some few looked withered and small; the hepatic cells in the centre of each lobule were nearly filled with bile pigment; the cancer cells appeared to have been, in many places, altogether confined to the interlobular connective tissue. The glands in the transverse fissure of the liver were infiltrated with cancer, and formed a tumour as large as a hen's egg, which embraced the common bile duct and the blood vessels. The trunk and primary branches of the vena porta, hepatic artery, and bile ducts, were all quite pervious, and free from coagula, and but little altered in size; the cystic duct, however, was much compressed, and superficially ulcerated at its origin from the gall-bladder, by one of these cancerous glands; the gall-bladder was contracted, its coats were thickened, and it contained about half an ounce of dark green bile. The head of the pancreas was slightly infiltrated with cancer, was hard and of a greyish-yellow colour, but the opening of the common bile duct into the duodenum was quite pervious. The right common iliac vein was much compressed by a large cancerous gland; its cavity did not contain any coagula, or cancerous masses; the iliac arteries were quite free and pervious. The stomach and intestines were normal; the spleen was small, weighing 4½ oz., its fibrous coat thickened, the parenchyma firm and of a

dark brown colour. Iodine and sulphuric acid did not develope any so-called amyloid reaction when applied to the tissue of the spleen, liver, or kidneys; the latter were small, hard, smooth, and congested, weighing only 3½ oz. each; the heart was very small, weighing only 5½ oz., its muscular tissue was pale and softer than natural, but microscopic examination did not reveal any fatty degeneration; the cardiac valves and orifices were healthy; the lungs were much congested, but, in other respects, quite normal. Dr. Cryan stated that it was worthy of notice that in this case there was complete absence of all pain, thus affording a further illustration of the clinical fact that encephaloid disease of the liver is often painless, and in marked contrast, in that respect, with true scirrhus of the organ, which is comparatively rare, but an interesting case of which has been recorded by Dr. Stokes—*Proceedings of Pathological Society*, New Series, Vol. iii., p. 170,—in which "the principal symptoms were jaundice and *intolerable and persistent pain.*" The great atrophy of the heart in the present case was also of interest, as well as the cause of obstruction to the circulation in the right common iliac vein revealed after death, and explaining the œdema of the right lower extremity noticed during the last ten days of the patient's life. In a case of cancer of the liver recorded by Dr. Gordon (*Proceedings of Pathological Society*, New Series, Vol. ii., p. 160), the patient suddenly complained of severe pain in the left lower extremity, rapidly followed by great swelling, and a fall of several degrees in its temperature, with absence of any tenderness on pressure over the large veins.—Dr. Gordon diagnosed that the main artery of the limb had been suddenly occluded, and *post mortem* examination verified the diagnosis, by showing that the interruption to the circulation was caused by the left common iliac artery being completely obstructed by a mass of semi-fluid cancerous matter.—*February* 20, 1869.

Rheumatic Inflammation of Pia Mater.—Dr. Little said, a fortnight ago a lad, aged eighteen, was brought into his ward in the Adelaide Hospital suffering from acute rheumatism. It was his third attack, he having had one four years, and another two years previously. He had been ill about a week before the date of his admission. It was a case of fibrous rheumatism, as distinguished from synovial rheumatism; that is, the joints were not swollen to any extent or painful, as long as he kept them perfectly at rest, but the least motion or manipulation caused severe pain. It was also that sort of rheumatism that moves from one joint to another. When admitted the left shoulder, wrist, knee, and ankle were attacked; whereas, for the first two or three days of his illness, he said it was the joints on the right side of the body alone that were affected. He presented the usual symptoms of rheumatic fever, except that the sweating was not so marked as it usually is; he had

retention of urine. On listening over the heart there was a distinct murmur to be heard, with the first sound most audible over the third and fourth left costal cartilages and the intervening space. During the week following admission the pains in the joints became less, the use of the catheter became no longer necessary to relieve the bladder, and some palpitation which he had about the heart ceased. On that day week, however, a change was noticed in his condition. On the previous evening the nurse said he was a little drowsy, and when Dr. Little saw him in the morning the drowsiness was very apparent. When roused up he appeared to be very weak; he fell over, and could not be kept awake for more than a second or two at a time. On examining the chest no alteration in the cardiac murmur was observed, except that it was not so distinct as before, probably because the heart's action was exceedingly rapid. Over the base of both lungs posteriorly there was dulness, particularly marked over the base of the left lung, where there was to be heard a friction sound, whiffing tubular breathing, and occasionally crepitus. The stupor became gradually more intense, and he died that night. On Monday last an examination of the body was made. On the thoracic cavity being opened about a pint and a-half of serum was found in the pleural sac; the lower lobe of the left lung was in a state of splenization; an ounce of serum was found in the sac of the pericardium, and in it floated one small shred of lymph. On opening the heart he found the murmur which had been observed was due to former disease. The appearances at the aortic opening were due to old disease, and doubtless had their origin in a previous attack, one segment of the valve being opaque, thickened, and corrugated.

In the ventricle was found a firm fibrinous clot, which presented evidence of having been formed during life, inasmuch as it was furrowed as if by the current of blood flowing into the aorta.

Thirty hours after death the blood flowed from the ascending vena cava with as much freedom as it would from a vein during life. It was alkaline, but not more so than blood should be; a quantity of it being placed in a glass it formed a thin small clot, not at all firm. The veins on the surface of the brain were unusually full of blood. On slicing the right hemisphere of the brain there was found in the lateral ventricle no departure from health, but on opening the left lateral ventricle they found three drachms of turbid serum, which, under the microscope, was seen to contain blood corpuscles and distinct lymph globules. Head symptoms coming on in cases of rheumatic fever were doubtless usually connected with inflammatory change in the heart, but occasionally, as in this case, they arose from the rheumatic poison attacking the structures within the skull. It was to be observed that it was the left lateral ventricle, corresponding to the side on which the joints were attacked, in which this turbid serum was found; and the finding of this turbid serum within the ventricle

was an illustration of the tendency of the rheumatic poison to attack certain structures. Fluid free in the cavity of the ventricle must have been derived from the pia mater, a fibro cellular membrane, and approaching therefore in structure to the tissues more commonly attacked.—*February* 20, 1869.

Abnormal Condition of the Red Blood Corpuscles.—Dr. Purser said:—I wish to bring under the notice of the Society, and so to place on record, a curious observation which I have recently made. On examining the blood of a frog into whose lymphatic spaces some milk had been injected about twenty-four hours previously, the white corpuscles were seen to contain, almost without exception, fat globules in variable number, some holding only one shining particle, while others were so gorged with oil drops as to resemble the colostrum corpuscles of milk. This was in no way surprising; but the red corpuscles also contained each one or more refracting grains, which at first sight closely resembled fat. On more careful examination, however, they were found to differ from fat in having a less refracting power, in being unacted on by ether while they dissolved slowly in distilled water, more readily in ammonia, and with great rapidity in acetic acid and caustic potash. They were hence of a proteinaceous nature. The greater number of corpuscles contained only two or three such particles, but as many as nine were counted in some. They lay about midway between the nucleus and periphery of the cell generally, were multiple in lines, but sometimes in small groups of three or more. The blood coagulated firmly, and did not differ in any other respect from healthy blood. A careful spectroscopic examination was made. The absorption bands were identical with those of normal hemoglobulin. I may mention that the frog died a short time after being made motionless with curara for the purpose of microscopic examination of the circulation in the mesentery. The blood was examined about four hours after death. I am quite unable to assign the import of this observation. That it was unconnected with the injection of milk I am convinced, for in many other cases in which I have performed this operation, although the white corpuscles were loaded with oil, the red corpuscles were unaltered. I have thought it worth recording, as I have never seen or heard of a similar case.—*March* 6.

Primary Cancer of the Liver.—Dr. John H. Benson said:—These specimens, Mr. President, were taken from the body of a man, aged seventy-one, who died under my care, in the City of Dublin Hospital, a few days ago. He stated that he had always been a healthy man up to about a year since, and that then he began to suffer from a sense of uneasiness and weight in the right side, which gradually increased up to

the date of his admission, but was at no time severe. He also stated that he had lived temperately for many years. However, I was told that he boasted to the other patients in the ward that he usually drank about nine glasses of whiskey before breakfast.

On admission, the patient complained of a "soreness," to use his own expression, "from the groin to the shoulder," and on examining the right side I found evidence of a very large liver. It extended downwards almost to the umbilicus. There was considerable tenderness over the whole liver, but it was especially marked in certain spots. On palpation a sense of hardness was communicated to the hand, but I found it impossible to say whether the surface of the liver was smooth or otherwise, owing to the tension of the recti muscles whenever pressure was made on the part. Notwithstanding this tenderness, there was a total absence of pyrexia; pulse 65, regular, but small; tongue slightly furred. No œdema existed anywhere, nor any acites. The urine contained a small quantity of albumen. Judging, then, from the absence of pyrexia, from the apparently slow development of the disease, from the history of his intemperate habits, and from the absence of signs of organic disease in any other viscus, I was induced to set the case down as one of either ordinary fatty, or of amyloid disease of the liver, and I treated it accordingly.

For nine days the patient progressed favourably, saying that he felt better from day to day. On last Monday, however, the fifth day from admission, he did not feel so well, and for the first time a slight jaundiced hue was perceptible over the body. On the next day the yellow colour of the surface was intensified; the urine was tinged with bile; pulse 104, small and irregular; respiration 25, deep and thoracic. He lay in a kind of torpor, but when spoken to he roused up and answered intelligently. This semi-comatose condition directed my attention to the kidneys, but I found that the urine was copious in quantity. On Wednesday all the symptoms were seen to have become considerably aggravated, and in the evening he sank and died.

On opening the abdomen, assisted by my colleague, Dr. Hewitt, we found from about four to six ounces of fluid, containing a large proportion of blood, in the cavity of the peritoneum. There was no evidence of peritonitis. Numerous petechial extravasations of blood, varying from one to three or four lines in diameter, were visible in the mesentery. The liver was found enlarged to twice its normal size, weighing 8 lbs. 5½ oz. The substance of the liver in parts presented somewhat the appearance of the amyloid disease; but, on applying the iodine and sulphuric acid test, no characteristic reaction took place. The greater portion of the liver, however, was of a purplish red hue. Besides this alteration, we discovered numerous tumours, varying from the size of a hazel-nut to that of a small orange. They were invariably raised above

the surrounding surface, and with a very few exceptions were of a much darker colour, and somewhat softer and more elastic to the touch than the surrounding structure. They were not depressed in the centre. When cut into, they presented a circular or oval section, with a well-defined margin, and their substance projected with a slight convexity, about equal in degree to that of a watch-glass. With few exceptions the diameters of the tumours when cut into were found to be considerably greater than that of their disc which appeared on the peritoneal surface of the liver. Dr. John Barker kindly examined one of these tumours, and pronounced it to be medullary cancer. On raising the liver, we found that the lymphatic glands in the transverse fissure were greatly enlarged, and were so soft that a slight pressure of the fingers reduced them to a soft grumous pap.

Suspecting these tumours to be cancerous at the time we discovered them, we next proceeded to look for cancer elsewhere. The stomach contained some dark, altered blood, but no cancer existed there. The intestines and the other abdominal organs were equally free. On opening the thorax, we discovered a number of enlarged glands on the right side exclusively of the anterior mediastinum.

In the apex of each lung we found a few old tubercular masses, which had undergone calcareous degeneration, and a very few small old cavities, but no trace of cancer was perceptible. However, while opening the pericardium, we discovered a tumour, about the size of a walnut, situated in the angle formed by the junction of the pericardium and diaphragm. This tumour I examined myself, and found it to be cancerous. A few enlarged glands were found in the right side of the neck. No other traces of cancer, besides those I have mentioned, were visible anywhere in the body.

The heart had undergone some fatty degeneration. A few calcifications were imbedded in the substance of some of the chordæ tendineæ of the left side, and in the left zona tendinosa. On the free edge of two of the tricuspid flaps we found a somewhat bulbous smooth swelling, yet these gave rise to no murmur during life.

The kidneys presented appearances due to Bright's disease, and in some places may be seen that appearance of the pyramids said to resemble a sheaf of wheat. On one of the kidneys we saw a large cyst, such as we sometimes find in cases of Bright's disease.

This case, Sir, is interesting, I think, as presenting an example of primary cancer of the liver, which we know to be rare, and as not exhibiting many of the most important diagnostic symptoms of that disease. The entire absence of cachexia, or of pyrexia, and the apparently slow progress of the disease, the suddenness of his death, and the absence of cancerous disease in other organs, are remarkable. The age of the patient, too, was somewhat unusual. Out of eighty-three cases

analysed by Frerichs with reference to age, only two exceeded seventy. This patient's age was seventy-one.—*March* 6, 1869.

Malformation of the Chordæ Tendineæ ; Congenital Musical Bruit.—Dr. Mayne said the specimen to which he wished to call the attention of the Pathological Society was one of considerable interest. It was taken from the body of a man named Edward Kelly, aged 69 years, who died some weeks before in the hospital of the South Dublin Union, under the care of his friend, Dr. Jennings, to whom he was indebted for permission to exhibit it here to-day. The case, he considered an interesting one, in two points of view, as affording an explanation of an exceedingly rare cardiac bruit ; and as also indicating the connexion which, he believed, frequently existed between obstructive cardiac disease and derangement of the cerebral functions.

In the year 1860, at which time his father, the late Dr. Mayne, was physician to the above institution, Kelly first came under medical notice. He read a very brief entry, made by Dr. Mayne in that year "March 24th.—A musical systolic bruit, most intense at the left apex, inaudible between the spine and scapula, pulse regular and full."

For a considerable period after this Kelly continued in good health, but subsequently he became subject to epileptiform convulsions, which occurred every three months, and occasionally to fits of mania, during which he was very violent and unmanageable. He had the well-marked symptoms of aortic disease for the last three or four years of his life, and he finally sank under the complications arising from the latter disorder.

The musical bruit first demanded attention. If the interior of the left ventricle were examined, a ready solution of the difficulty would be discovered. Two irregular chordæ tendineæ would be observed, which stretched across the area of that chamber, one from the posterior wall to the zona tendinosa, which surrounded the aortic opening, and the other from the former to the anterior wall.

Those present who were familiar with the anatomy of the heart, would at once perceive that this arrangement was most irregular, as the chordæ tendineæ normally extended from the musculi papillares on the sides of the ventricle, to the corresponding divisions of the mitral valve, and in no instance which he could discover had they been known either to cross that chamber, or to be attached to the aortic opening.

He had no hesitation in stating that the blood in its passage over the first of these cords produced the strange musical note ; and he believed it was totally unconnected with the condition of the aortic valves, which had undergone, as might be perceived, osseous degeneration, and had converted the aortic opening into a mere chink.

The bruit was unquestionably congenital. It was remarkable also in being *systolic*, and in being most intense at the apex.

In the other recorded cases with which he was familiar, the sound was diastolic, and was most audible at the base. The attachment, however, of this irregular cord would easily explain the direction of its intensity.

Dr. Banks, in the 4th Vol. of the *Dublin Hospital Gazette*, described a case of musical bruit, where the sound, a "single regurgitant one," was most audible at the base, and was produced by three rudimentary valves, each placed about $\frac{1}{4}$ of an inch below the aortic opening, and attached to the sides of the ventricle. These occupied pretty much the same position as the irregular cord in the heart exhibited. Dr. Stokes mentioned an instance where a similar sound was produced by an osseous infiltration of the commencement of the aorta. Sir D. Corrigan knew a tongue of bone $\frac{1}{4}$ of an inch in length, projecting into the same vessel to elicit a like phenomenon.

Some present might regard the disease of the aortic valves as the true cause of the bruit, but he had been influenced to form a different opinion from the following considerations:—

1st. That the sound existed years before there were any other signs of aortic disease.

2ndly. That it was utterly impossible for the blood to escape from the ventricle into the aorta without producing a vibration of the tense cord stretched across that aperture, which must have been accompanied by a sound of more or less intensity.

3rdly. That the sound was most audible at the left apex, exactly corresponding to the inferior attachment of this irregular cord.

He was inclined to attribute to the extreme obstruction of the aortic orifice, and the consequent deficient vascular supply of the brain, the epileptiform convulsions, and maniacal seizures with which this case was complicated.

He thought these sequelæ of cardiac affections had not as yet been sufficiently recognized by the profession.—*March* 6, 1869.

Hypertrophy of the Heart.—Dr. Grimshaw exhibited a case of an enormously large heart taken from the body of a patient who had been under the care of Dr. Burke in Steevens' Hospital. The man was forty years of age, and a bricklayer. He had been ill twelve months before admission to hospital. He gradually became worse, and was admitted on the 19th of February. The chief point of interest was the enormous pulsation of the heart. The whole chest seemed to move in front with the force of the pulsation. There was great dyspnea and some œdema of the lower extremities. He had never had rheumatic fever. There was of course an enormously increased extent of precordial dulness, and Dr. Burke detected a distinct murmur with the second sound of the heart. The patient died on the 21st of February. On a *post mortem* examination

all the cavities of the heart were found enormously thickened. He had seldom seen them so thick. The only valvular disease to be detected was that of the aortic orifice. The valves were extensively diseased, and the aorta enormously dilated.—*March* 6, 1869.

Hypertrophy of the Heart.—Dr. MacSwiney said—Four weeks ago, a man between sixty and seventy years of age, applied to me for medical advice. He complained of extreme distress of breathing, great palpitation of his heart, and a distressing cough. I admitted him to Jervis-street Hospital, and upon instituting a careful examination of his chest, I found the area of precordial dulness considerably increased, and I also found a perceptibly increased cardiac impulse. He had irregular pulse, and plain evidence of bronchitis in both lungs. He was put under treatment which did not materially improve his condition. On the contrary, he gradually became weaker, and ultimately died. One week before he died he expectorated large masses of black blood. Perhaps half a dozen or a dozen of these sputa would be expectorated in the day, and corresponding with the symtoms I have described there appeared at the base of the right lung—rapidly extending upwards—dulness on percussion. He died asphyxiated, or, strictly speaking, by apnea. The parts which I now exhibit consist of the lungs and the heart. The left lung was to all seeming healthy. The right was the subject of most extensive pulmonary apoplexy, of that character where the pulmonary tissue is infiltrated throughout with blood. The heart was found to be very considerably hypertrophied, and the hypertrophy was confined exclusively to the left ventricle. It was simple hypertrophy, being neither eccentric nor concentric in any remarkable degree, but being an increase of the muscular structure of the fibres of the left ventricle, without any contraction or dilatation of that chamber. I omitted to remark that there was no abnormal bruit audible during life in the precordial region. The sounds were perfectly normal on the several occasions I examined him; notwithstanding this, I confess I expected to find evidence of valvular disease. None such, however, existed. The aortic valves were perfectly sufficient, and quite competent to retain a column of water when poured into them, as may now be seen. The mitral valves were healthy in every particular. The auriculo-ventricular opening was normal. The kidneys were normal in every respect, save that they were the subject of very considerable sanguineous congestion. Similarly the liver exhibited no trace of disease cognizable by the senses. When cut into, it also gave vent to a great mass of black blood. This was explained by the circumstances that attended the form of death in this instance; it being by apnea it was to be expected that the right chambers of the heart would be occupied with black blood, and all the organs essential to life congested with dark blood.

To those who specially devote their attention to cardiac and pulmonary

diseases there are two features of considerable interest in this case. Firstly, there is the occurrence of pulmonary apoplexy without any valvular disease of the heart. Perhaps it will be found on investigation that instances in which pulmonary apoplexy has been recorded to have existed, are extremely rare in which there was no mitral disease. The second point of interest is the existence of this remarkable hypertrophy of the wall of the left ventricle, more than an inch or an inch and a half in thickness, without a trace of aortic insufficiency or other valvular disease. It is known that the existence of aortic valve disease is the most common cause of the occurrence of this lesion; and I am not aware that there are many instances recorded where it has been found present, and where no organ of the body exhibited any evidence of structural disease. Anything which causes constant excitement of the heart's action is competent to cause enlargement of the muscular structure of the heart. Now, this man was an habitual drunkard for thirty or forty years, and the hypertrophy may be to some extent due to constant intemperance.

It is proper to remark, however, that when the aorta was split open, it showed evidence of atheromatous disease but trifling in amount. Now we know that the presence of atheroma affecting the vessels is capable of interfering with their elasticity, and anything capable of interfering with the elasticity of the blood vessels is capable of producing hypertrophy of the left ventricle in a degree proportionate to the extent and duration of the lesion of vascular tissue.—*March* 13, 1869.

Wound of the Heart.—Dr. Fleming exhibited the heart of a soldier in the Royal Artillery, who had committed suicide by stabbing himself a few days previously. It appeared that the man had been intemperate, and, owing to his excesses, shortly before his suicide, he had become completely unmanned. He had been heard to speak of the uneasiness which he experienced, and also of putting an end to his existence. On the occasion of one of these attacks of intemperance he was asked to go to his room, which was adjoining the apartment in which he was at the time. He said he dreaded the idea of going to bed, that his sensations were frightful, and he even at that time mentioned that he would put an end to himself. He spoke to the wife of the sergeant, and she suddenly heard a click like that of a penknife. She seized his arm, he pulled the knife forcibly out of his body, laid it deliberately on the table, and said, "Now I should like to be placed where I can die with ease." Ten minutes afterwards he was dead. Dr. Cuff saw him very soon after the occurrence; he was then pulseless, in a state of complete collapse, able to lie horizontally, and inclined to the right side. His breathing, as described by Dr. Cuff, was suspirious; he would draw two or three breaths, then cease, and after irregular intervals resume the act of inspiration.

Along with the heart, he now exhibited a small portion of the integument over the apex, in order that they might see the very slight wound that had reached and penetrated the heart. There was hardly any external bleeding. The amount of hemorrhage stained the shirt in the neighbourhood of the wound, but not more than was sufficient to attract attention, and no discharge of blood took place subsequently. The depth to which the knife had penetrated was nearly three inches, and it was supposed he took the knife in his left hand, suddenly opened and drove it into the heart. It was found that he had perforated the heart at its apex, the knife having been driven through the left ventricle. The pericardium was distended with blood, partly fluid, and partly coagulated. The pleura also contained a great quantity of blood. There was an enormous amount of fat deposited on the heart, but there was no fatty degeneration.—*March* 20, 1869.

Cystic Tumour of the Breasts.—Dr. Robert M'Donnell exhibited to the Society a tumour, weighing about two pounds, which he had removed on November 23. It was, in fact, a recurrence of the disease in the patient whose case he had brought under the notice of the Society a year ago (5th December, 1868).

It was unnecessary to refer to the early history of the case, as this was already detailed in the records of the Society; suffice it to say, that the original tumour had been a very large sarcomatous tumour of the breast, of the variety known as myxoma. It had recurred, as these growths unfortunately often do, and for six weeks previous to the second operation had increased rapidly in size. There was no reason to change the opinion formed as to the nature of the disease; although recurrent, there was no more evidence of its being malignant, in the true sense of the word, than at the time of the first operation. The patient's general health was excellent, and there was not any trace of glandular enlargement in the axilla. The rapidity of its growth during the last few weeks made its removal necessary.

The structure of the recurrent growth was identical with that of the original tumour. No cysts, however, had formed, but the soft "colloid" structure of the mass yielded a larger quantity of gummy juice; a small portion of the tumour, when bruised up in a mortar with silver sand, and treated with cold water, yielded on filtration a fluid thick as flax-seed tea, and containing abundance of mucin, the presence of which Dr. M'Donnell demonstrated to the members present by its peculiar reaction on the addition of acetic acid.—*November* 27, 1869.

Suppuration of the Eyeball after Injury.—Mr. Wilson exhibited an eye removed from a mechanic, who, two years previously, received an injury

of it from a piece of metal, in consequence of which the eye became blind. About the 1st October this blind eye was again injured by hot metal striking it, in consequence of which the organ became exceedingly swollen and painful, and the man was unable to obtain any rest or sleep for a long time. When Mr. Wilson saw him, five weeks subsequently, there was considerable chemosis and pain, and the vision of the other eye was somewhat impaired; slight lachrymation and intolerance of light were also present in this eye.

To save the uninjured eye, as well as to relieve the great suffering, Mr. Wilson removed the eyeball, and on opening it found the space, recently occupied by the vitreous humour, filled with pus, the retina detached and funnel-like, attached behind only to the optic nerve, and in front to the ciliary region. The space between the retina and the choroid was filled with a sanguineous fluid; the choroid itself was healthy. There was a large mass of firm neoplastic tissue at the ciliary region, which would account, in a great measure, for the pain and suffering, as well as for the sympathetic affection of the other eye.—*November* 27, 1869.

Rupture of the Vena Cava.—Mr. Wharton exhibited an example of rupture of the vena cava, close to the diaphragm, together with a rupture of the small intestine, caused by the passage of the wheel of a car over the abdomen. The patient was a child, aged twelve, and was brought to the Meath Hospital at once. The injury was pronounced to be a ruptured intestine. There was great and constant desire to pass water, but when a catheter was introduced, not more than half an ounce of urine flowed through it. The abdomen, in appearance, somewhat resembled that observed in cases of distended bladder. There was no fracture or mark of violence externally. Death took place at the end of twenty hours after the receipt of the injury. The quantity of blood effused was not very considerable, in consequence, most probably, of the syncope which ensued, the small size of the rent in the vessel, and the formation of a clot at the aperture. The largest quantity of blood was found under the right kidney. The peritoneal cavity contained a large quantity of serum. Mr. Wharton observed that Professor R. W. Smith had, many years ago, reported a case, in which the superior vena cava had been torn completely from the right auricle in a woman, whose chest was violently crushed between a heavily loaded cart and a wall, but there was no fracture of any part of the osseous walls of the thorax.—*November* 27, 1869.

Fracture of the Ribs.—Mr. Hamilton gave the following history of this case:—

A healthy man, twenty-five years of age, fell from a scaffold twelve feet high. He was brought to the Richmond Hospital in a complete

state of collapse. On examination, it was found the left clavicle was broken, and the fragments so sharp that they had almost perforated the skin. The ribs were broken on the left side, and driven in so that the hand could be passed into the depression. There was a good deal of difficulty of breathing, and a constant spitting of blood. During the night the hemoptysis continued, the blood being partly fluid, and partly clotted. He passed also bloody urine. He went on pretty well until the third day. On the night of that day he became affected with a most dreadful pain in the region of the left kidney, and after a time he passed a very large quantity of dark fluid blood. For some few days afterwards there was considerable pain in the region of the kidney, but it subsided, and the bloody urine and the hemoptysis ceased. The pulse came down to 80, and the man showed signs of decided improvement. After the lapse of a fortnight, having got up to go to the night chair, he suddenly became quite pallid, was bathed in a profuse perspiration, his lips became perfectly livid, and he died before any assistance could be given him.

The following were the results of the *post mortem* examination: There was a comminuted fracture of the clavicle, with overlapping of the fragment. Seven ribs on the left side were found to have been broken near their posterior angles. They were very fairly in apposition, and there were no spiculæ. The left lung had become closely adherent to the ribs by lymph, effused along the line of the fractures, and in the vicinity. In two places the broken ribs had perforated the surface of the lung, but the perforations were exceedingly small, and scarcely sufficient to account for the great hemoptysis which had occurred during the progress of the case. When the hand was placed on the lung therewas found to be a substance in it almost as large as an orange, and when the lung was cut across, it was found that this was a firm coagulum of blood which filled a rent of the upper lobe. In the lower lobe, also, there was a laceration of the pulmonary tissue, and the lung was congested with blood. This state of the lung, it was obvious, arose from the great violence with which the man had struck his side in the fall, breaking seven ribs, and crushing the lung. He apprehended it was one of the original vessels that had broke out from the lung in the first instance that had given way, and this was rendered the more likely by what occurred, for the man smoked a pipe, got up, and went to the night chair, when, to use his own words, he felt as if his heart-strings gave way, and a quantity of blood poured out. During life the man had passed bloody urine, and complained of the kidney on the left side, and it was easy to diagnose that the kidney had been crushed by striking against some projecting part of the scaffold. They found in the cellular tissue around the kidney a quantity of extravasated blood. The interior of the organ presented a similar condition, owing to the rupture of its vessels by the violence of the contusion.—*December* 4, 1869.

Hepatic Abscess.—Mr. H. G. Croly said the specimen which he now laid before the Society was one of abscess of the liver, removed from the body of a man, aged forty-one, who had been admitted to the City of Dublin Hospital, on the 5th of November, suffering from phthisis. He stated that he had been first attacked with illness two years back. On admission he was greatly wasted and emaciated, but till the day previous to his death, he declared he felt himself improving and getting stronger. It was not until the day before his death he complained of a serious change. He died on the 24th of November. His chief symptoms were wasting, night sweats, cough, and a certain amount of pain under the right clavicle. They could not discover any extensive disease in the lungs, but looked on the case as one of scattered tubercles. He died suddenly. Half an hour before his death Dr. Croly saw him. He got up and dressed himself, when he suddenly fainted, and then became covered with a cold clammy sweat. Restoratives were exhibited, but they failed to produce any effect. He never rallied, and soon expired.

Upon laying open the chest its contents were found to be healthy, with the exception of a few tubercles in the upper part of the right lung. On opening the abdomen to obtain the liver for exhibition (for during life they had examined this organ frequently, and believed it to be an example of the fatty liver of phthisis), a large abscess suddenly burst, which had occupied the entire of the right lobe. The matter dicharged from this abscess filled the abdominal cavity in a few seconds. It was extremely fetid, and contained a number of large flakes. It presented all the appearance of strumous matter coming from a large chronic abscess. He supposed this abscess gave way suddenly, but he could not say whether it had given way before he had raised the liver out of its position, or during the separation of the liver from the diaphragm, to which the upper portion was adherent. It was important to remark, that the man had no diarrhea at any time, or anything to lead them to suppose he had ulceration of the bowels.—*December* 4, 1869.

Suicidal Fracture of the Larynx.—Mr. W. Stokes, jun., laid before the Society a specimen showing a comminuted fracture of the thyroid cartilage. On the 20th of July last, a man was brought into the Richmond Hospital suffering from a wound in the throat, which he had inflicted on himself with a large rusty bread knife, causing a gaping chasm, so wide that they could get a complete view of the vocal apparatus; the true and false vocal chords could be distinctly seen moving during respiration and attempts to speak. They could also, on looking upwards, see the epiglottis rising and falling during the acts of respiration. When the patient had his head thrown backwards and attempted to speak, the articulation was extremely indistinct, but there was this remarkable phenomenon connected with it—a double voice—one voice appeared to

come through the wound, and the other through the mouth, each differing in character. When the head was thrown forwards, the articulation became distinct. There was no hemorrhage, although there must have been a great deal before he came into hospital, for his clothes were saturated with blood. The patient stated that he had been a man of intemperate habits for years, sometimes drinking for weeks together without intermission, and he suffered from great depression of spirits, and had many times thought of taking away his life. He had, in fact, made two previous attempts to do so. Once at Greenock he threw himself off the pier into the river; and on the second occasion he attempted to cut his throat; the third and successful attempt was the result of a sudden impulse. He had no idea of it, he said, a few moments before inflicting the wound. It was an instantaneous action, and done without a moment's reflection. Everything that was done for the man proved to be quite unavailing, owing to his very restless condition. On the evening of the second day he got out of bed, and roamed about the house, and the patients thought he was seeking for some instrument to complete the work he had begun. For the third and fourth days the patient appeared to be going on better, but on the fifth day his breathing became stertorous, and he seemed to be sinking. Under suitable treatment he rallied from this state, but on the afternoon of the 6th day he became worse; his pulse was 70, extremely weak and irregular; respiration became very rapid, and his mind wandered.

The following note was taken by the clinical clerk at 9.30 p.m.:— "Auscultation reveals a sound above the chest like to that of a car moving at a distance. The inspirations are almost equal in number to the heart's impulse, viz., 76 to 80 in the minute. He says he is suffering great pain all over the chest, and in the region of the liver and stomach."

Such were the principal points connected with the case. It was one of interest, as having afforded an opportunity of viewing the entire of the vocal apparatus better than could be obtained by any laryngoscope, and as showing the great length of time a patient could live after the infliction of so formidable an injury. The double voice, the peculiar respiratory sound, the rapidity of inspiration, and the absence of any pulmonary congestion or inflammation (notwithstanding the exposure to which the patient subjected himself by wandering about the hospital), were also a feature of interest and importance.

The wound passed immediately before the superior margin of the thyroid cartilage, and the man must have made several gashes with the knife, for the thyroid cartilage (partly cut and partly broken) was divided into seven pieces. The posterior wall of the larynx was opened into the pharynx, showing the true and false vocal chords.—*December* 4, 1869.

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PERIODICALS WITH WHICH THE DUBLIN QUARTERLY JOURNAL IS EXCHANGED.

GREAT BRITAIN.

1. The British and Foreign Medico-Chirurgical Review. Churchill.
2. The Edinburgh Medical Journal. Oliver and Boyd.
3. The Retrospect of Medicine. Edited by W. Braithwaite. Simpkin and Co.
4. The Half-yearly Abstract of the Medical Sciences. Churchill.
5. Pharmaceutical Journal. Churchill.
6. The Lancet.
7. The Medical Times and Gazette. Churchill.
8. The British Medical Journal.
9. The Asylum Journal of Mental Science. Churchill.
10. The Glasgow Medical Journal. Mackenzie.
11. The Athenæum.
12. The Dublin Medical Press.
13. The Westminster Review. Trübner.
14. Transactions of Obstetrical Society. London: Longmans.
15. Journal of Cutaneous Medicine and Diseases of the Skin. Edited by Erasmus Wilson, F.R.S. Churchill.
16. The Practitioner; a Monthly Journal of Therapeutics. Macmillan and Co.
17. The Journal of Anatomy and Physiology. Macmillan.

INDIA.

18. The Indian Annals of Medical Science. Calcutta: Lepage and Co.
19. Transactions of the Medical and Physical Society of Bombay.
20. The Madras Quarterly Journal of Medical Science. Madras: Gantz, Brothers.
21. Indian Medical Gazette. Calcutta: G. Wyman and Co.

AUSTRALIA.

22. The Australian Medical Journal, Melbourne: Wilson and Mackinnon. London: H. Baillière.

AMERICA.

23. The American Journal of the Medical Sciences. Edited by Isaac Hays, M.D. Philadelphia: Blanchard and Lea.
24. The American Journal of Science and Arts. Conducted by Professors Silliman and B. Silliman, Jun., and J. D. Dana, &c. New Haven.

AMERICA.—*Continued.*

25. The American Journal of Insanity, Utica, N.Y. State Lunatic Asylum.
26. The American Journal of Obstetrics and Diseases of Women and Childbed, New York: Moorhead, Bond, & Co.
27. The Cincinnati Lancet and Observer. Cincinnati.
28. Canada Medical Journal. Montreal: Dawson, Brothers.
29. The New York Medical Journal. New York.
30. The Philadelphia Medical and Surgical Reporter.
31. The Richmond and Louisville Medical Journal.
32. The Medical Record. New York: Wood & Co.
33. The New Orleans Journal of Medicine. Trübner.
34. The Western Journal of Medicine. Indianapolis, Ind., U.S.A.: C. D. Cazenove, London.

FRANCE.

35. Gazette Médicale de Paris. Paris
36. Gazette Hebdomadaire de Médecine et de Chirurgie. Paris: Victor Masson.
37. Journal de Chimie Médicale, de Pharmacie, de Toxicologie, et Revue de nouvelles scientifiques, nationales et étrangeres, &c. Paris: Labé.
38. Journal de Médecine de L'Ouest. Nantes: Mellinet.
39. Journal de Pharmacie et de Chimie, &c. Paris: Victor Masson.
40. L'Union Médicale. Paris.
41. La Lancette Française, Gazette des Hôpitaux civils et militaires. Paris.
42. Revue Médicale Française et étrangère. Publié par le Docteur Sales-Girons, Paris.
43. Archives Générales de Médecine. Paris: Asselin.
44. Bulletin de l'Académie de Médecine. Paris: Baillière.
45. Mémoires de l'Académie de Médecine.
46. Revue de Thérapeutique Médico-Chirurgicale. Paris: Dr. A. Martin-Lauzer.
47. Journal de Médecine et de Chirurgie Pratiques a l'Usage des Médecines. Par Lucas-Cham-pionnère. Paris.
48. Journal des Connaissances Médicales et Pharmaceutiques. Paris.

FRANCE.—*Continued.*

49. Annales Médico - Psychologiques. Par MM. Baillarger, Cerise, et Moreau. Paris: V. Masson.

50. Bulletin Général de Thérapeutique, Médicale et Chirurgicale. Par le Docteur Debout. Paris.

51. Répertoire de Pharmacie. Par M. le Dr. Bouchardat.

52. Gazette Médicale de Strasbourg.

53. Journal de Médecine de Bordeaux.

54. L'Union Médicale de la Gironde, Bordeaux.

55. Annales D'Hygiène Publique et de Médecine Légale. Paris.

56. Gazette Médicale de Lyon. Dirigée par le Dr. P. Diday.

57. Montpellier Médical Journal mensuel de Médicine. Montpellier: Boehm et Fils.

58. Journal de Médecine Mentale. Par M. Delasiauve. Paris: Masson et Fils.

59. Archives de Médecine Navale. Paris: Baillière.

60. Mémoires et Bulletin de l'Académie Royale de Médecine de Belgique, Brussels.

BELGIUM.

61. Annales D'Oculistique. Brussels.

62. Annales et Bulletin de la Société de Médecine de Gand.

GERMANY.

63. Zeitschrift für rationelle Medicin. Herausgegeben Von Dr. J. Henle und Dr. C. v. Pfeufer. Heidelberg and Leipzig: C. F. Winter.

64. Vierteljahrschrift für die praktische Heilkunde, herausgegeben von der medicinischen Facultät in Prag. Prague: Karn André.

65. Canstatt's Jahresbericht über die Fortschritte der gesammten Medicin in allen Ländern. Redigirt Von Pr. Scherer, Pr. Virchow, und Dr. Eisenmann. Würzburg: Stahel.

66. Wochenblatt der Zeitschrift der k. Gesellschaft der Aerzte in Wein (Beilage zu den Jahrbüchern). Redigirt von A. Duchek, C. Langer, A. Schauenstein. Leipzig: Hinrichs.

67. Deutsches Archiv. für Klinische Medicin. Erlangen: Th. Blaesing.

68. Jahrbuch für Kinderheilkunde und Physische Erziehung. Leipzig: B. G. Trübner.

PRUSSIA.

69. Archiv für pathologische Anatomi. und Physiologie, &c. Herausgegeben voe R. Virchow. Berlin: G. Reimer.

70. Allgemeine Zeitschrift für Psychiatrie und psychisch-gerichtliche Medicin. Herausgegeben von Damerow, Flemming, Roller durch Heinrich Laehr. Berlin: Hirschwald.

71. Berliner Klinische Wochenschriftn Berlin: Hirschwald.

72. Archiv für Klinische Chirurgie Herausgegeben von Dr. B. von Langenbeck. Berlin: Hirschwald.

73. Monatsschrift für Geburstkunde und Frauenkrankheiten. Berlin: Hirschwald.

HOLLAND.

74. Archiv für die Holländischen Beiträge zur Natur-und Heilkunde, Herausgegeben von F. C. Donders, Utrecht, und W. Berlin, Amsterdam, Utrecht: C. Van Der Post.

NORWAY.

75. Norsk Magazin, for Lægevidenskaben. Udgivet af det medicinske Selskab i Christiania. Redigeret af W. Boeck. A. W. Münster. Lund: Voss. Christiania: Feilberg and Landmark.

SWEDEN.

76. Hygiea, Medicinsk och Farmaceutisk Manads-skrift. Stockholm.

DENMARK.

77. Bibliothek for Læger. Udgivet af Direktionen for det Classenske Literaturselskab. Redigeret af Dr. K. Brüniche. Copenhagen: Reitzels.

78. Hospitals-Tidende. Optegnelser af praktisk Lægekunst fra Ind- og Udlandet.

ITALY.

79. Bulletino delle Scienze Mediche. Publicato per cura della Società Medico-Chirurgica di Bologna.

80. Giornale Veneto di Scienze Mediche.

81. Lo Sperimentale Giornale Critico di Medicina e Chirurgia per servire ai Bisogni dell 'Arte Salutare. Direttore Prof. C. C. M. Bufalini. Florence.